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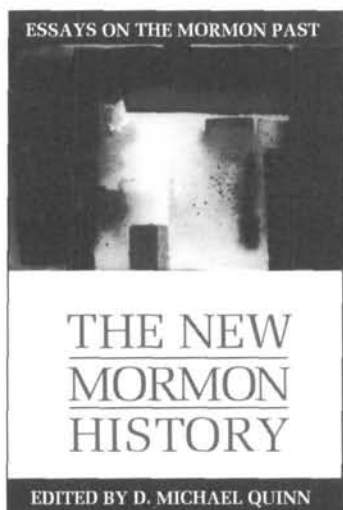
The American Historical Review

AMERICAN HISTORICAL ASSOCIATION



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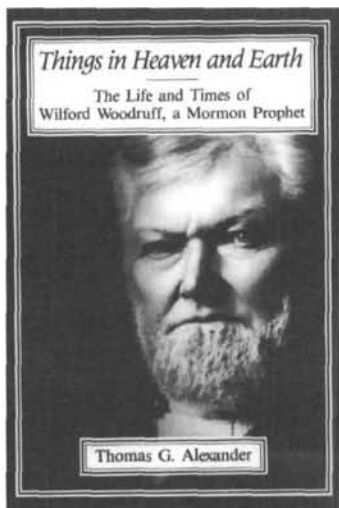
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In This Issue

The Modern Middle East

The genesis of this issue on the historiography of the modern Middle East was the recent war in the Persian Gulf. Many of the people attending last year's Annual Meeting of the AHA in New York City were alarmed at the threatened outbreak of major war in the region and concerned about how little many of us knew about the background to the conflict. Blanche Wiesen Cook, vice president of the Research Division of the AHA, brought to the meeting of the governing Council on December 30 a proposal generated at an impromptu meeting of over 200 historians that was organized on December 28 at the convention by graduate students from Rutgers University. The proposal, issued in the name of Historians Against the Persian Gulf War, recommended a number of educational projects that the AHA might undertake (see *Perspectives*, March 1991, page 14). One of these was a special issue of the *AHR* devoted to the Middle East.

I was eager to act on this proposal and went to work immediately in discussions at the AHA convention with scholars and *Review* staff. The role of the *AHR*, it was agreed, should not be to print information on the current state of the Middle East but rather to illuminate the state of historiography on the region in order to acquaint our readers with the key issues in its modern history, the methods and political and theoretical positions with which they were investigated. It would be important as well to engage the talents of leading specialists to produce essays that offered accurate and balanced assessments. A question arose as to whether we should take a country-by-country approach or focus on particular themes, such as pan-Arabism and Islamic fundamentalism. We decided to do both, because some of the scholars we hoped to engage focused on the histories of individual countries and others on issues affecting the entire region. We thought it best to let them treat subjects in ways that naturally arose from their research rather than to set an agenda of our own.

In my letter of invitation to the participating scholars, I pointed out that the principal purpose of the essays should be to inform our readers about important historical issues and their treatment and that the essays should direct our readers to scholarly works that would allow them to deepen their knowledge of Middle Eastern history. The authors could do this either through a review essay or through a survey of key historical issues with citations to and comment about books and articles of importance in the footnotes. Each of these options is represented in the essays that follow, although most of our authors adopted a combination of the two approaches.

The essays make clear how much work remains to be done in Middle Eastern history. For several countries and issues, scholarly inquiry has not moved beyond the use of diplomatic papers and personal memoirs. The secondary literature is still very small in quantity, and much of it exists only in the form of doctoral dissertations deposited at British and American universities.

I want to express special thanks to Aron Rodrigue, formerly of Indiana University and now at Stanford University, for helping me identify leading scholars in Middle Eastern studies. Others who contributed valuable advice and assistance include Derek Penslar (Indiana University), Rashid Khalidi (University of Chicago), Philip Khoury (Massachusetts Institute of Technology), Roy Mottahedeh (Harvard University), and Kenneth Stein (Emory University).

Finally, I should note that our book review section in this issue carries more reviews than usual on the Near East, including reviews of Bernard Lewis's *Political Language of Islam* and Jeremy Wilson's *Lawrence of Arabia: The Authorized Biography of T. E. Lawrence*.

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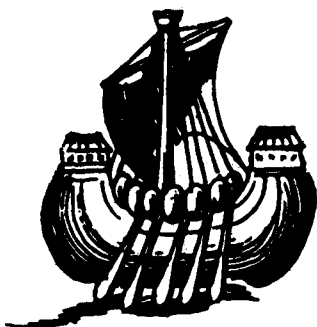
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Arab Nationalism: Historical Problems in the Literature

RASHID KHALIDI

AS WITH MANY ASPECTS OF MIDDLE EASTERN HISTORY, the study of Arab nationalism has tended to remain isolated from broader trends in history and the social sciences and specifically from the comparative study of nationalism. Similarly, most writing on nationalism has drawn sparingly on Middle Eastern examples. Thus, while a few of the early studies of nationalism in comparative perspective, such as that of Hans Kohn, devoted some attention to the nascent nationalisms of the Middle East including Arab nationalism, more recent writers, such as Eric Hobsbawm and Benedict Anderson, have touched on the Middle East only in passing, if at all.¹

Within the field of modern Middle Eastern history, beyond a general isolation from current trends in history, including the comparative study of nationalism, there has been a propensity toward compartmentalization along linguistic and national lines.² This has led to an unfortunate situation in which those studying Arab and Turkish nationalism, for example, have often been unaware of the relevance of one another's work, unfortunate because Middle Eastern nationalisms—such as Turkish and Arab nationalism before World War I or Zionism and Palestinian nationalism more recently—have strongly influenced one another in many ways and have served as the channels through which political concepts and forms of organization originating in Europe entered the Middle East. Failure to examine these reciprocal influences has at times led to an overemphasis on direct European influences and to numerous other kinds of distortions, notably a

¹ As can be seen from the title of his first book, *A History of Nationalism in the East* (New York, 1929), Hans Kohn came to the comparative study of nationalism after examining its specific properties in the Middle East and Asia, an examination that informs his book *The Idea of Nationalism: A Study in Its Origins and Background* (New York, 1944). See also E. J. Hobsbawm, *Nations and Nationalism since 1780: Programme, Myth, Reality* (Cambridge, 1990); Benedict Anderson, *Imagined Communities: Reflections on the Origins and Spread of Nationalism*, rev. edn. (New York, 1991). Hobsbawm offers a list of recent works on nationalism (p. 4), including Ernest Gellner, *Nations and Nationalism* (London, 1983), Anderson's book, and the collection edited by Hobsbawm and Terence Ranger, *The Invention of Tradition* (Cambridge, 1983). None of these books deals in any depth with Middle Eastern nationalisms. One of the few authors on nationalism with expertise in the Middle East field is Elie Kedourie, whose work is discussed below. See especially his *Nationalism*, 4th rev. edn. (London, 1985) and the edited work *Nationalism in Asia and Africa* (New York, 1970).

² This compartmentalization may be due in part to the specialized language study necessary for work on the historiography of Arab, Turkish, and Iranian nationalism, as well as Zionism. It is also in part a function of the insidious influence of nationalist rhetoric on these different historiographies. As a result of the heady impact of the subject studied on some of those studying it, much research and writing on these topics has come to reflect in microcosm the antagonisms between Middle Eastern nationalisms.

downplaying of the regional context in which each of these national movements developed.

The study of Arab nationalism in particular has suffered from these and other problems. Specifically, there has frequently been a failure to analyze the Arab case using methodologies derived from some of the more critical recent approaches in the study of nationalism. In the work of some scholars, this failure has been linked with an unquestioning acceptance of elements of the national narrative—or myths, to put it more bluntly—generated by Arab nationalism. The fact that the study of other Middle Eastern nationalisms is often plagued by the same flaws (often for the same reasons, growing out of the recent emergence of these national movements and the involvement of many of these scholars in the definition and support of their new polities) can be of little consolation to those interested in a balanced approach to the subject.

As historians with even a passing acquaintance with the field are aware, this failure means that treatments of the history of the Middle East, especially the history of the modern Middle East, are often partisan and controversial. Even scholarly writing on this subject is sometimes influenced by current political preoccupations to a degree greater than in many other fields. At the same time, political and public discourse in this country is frequently studded with citations of what are taken to be “the lessons of history” in the Middle East by commentators who are, as a rule, blissfully ignorant of the slightest knowledge of the history, languages, and cultures of that region. It is doubly unfortunate that this should be the case at a time of intense and growing interest in this field, an interest sometimes hard to sustain because of the clouds of polemics that surround it, particularly at moments of crisis in the Middle East.

Notwithstanding, it is possible to make a selection of seminal works in English on Arab nationalism and other aspects of modern Arab history that bear on it (the rich literature of works in Arabic and other languages will not be referred to here). These English-language works can be read with benefit by the nonspecialist, referred to in order to obtain a comparative perspective on other regions of the world, or provided to students interested in Middle Eastern history and politics. They can be grouped into two broad areas: the earliest works in the field and more recent books on Arab nationalism, including related works on specific nation-state nationalisms in the Arab world that shed particular light on the subject. As a preface to a discussion of these works, and to the key historical questions they deal with, a few introductory words on Arab nationalism are in order.

ARAB NATIONALISM, like most other Middle Eastern nationalisms, was a child of the intellectual atmosphere of the nineteenth century and one of many responses to the process of incorporation of the world into a single system with Europe at its center which that century witnessed. Like these other ideologies, Arab nationalism in its fully developed form represented an expression of identity and of group solidarity within the projected new format of the nation-state by an amalgam of

old elites and new social forces at once desirous of seeing their society resist control by outside forces and deeply influenced by the example and the challenge of the West.

Arab nationalism represented both a revival of old traditions and loyalties and a creation of new myths based on them, an invention of tradition, to use Hobsbawm and Ranger's term. Thus, as Arab nationalism took hold, what had been described for thirteen centuries as the glories of Islamic civilization came to be called the glories of Arab civilization; the language and literature of the Arabs, always revered and cherished, took on a new and heightened importance; and a sense of pride in Arabism that had always existed but had long been dormant was consciously revived and actively fostered.

By some time early in the twentieth century, at the end of this process of synthesis (which in many respects closely followed the pattern laid down by other national movements described by Anderson in *Imagined Communities*), the idea was widespread throughout the "Arab world" (itself a concept born of the rise of Arab nationalism) that anyone who spoke Arabic, looked back on the history of the Arabs with pride, and considered himself or herself to be an Arab was one, and that this sense of shared identity should in some measure find political expression. Soon, with the power of the state propagating it through the educational system, the media, and other avenues of access to cultural and political discourse in a number of newly independent Arab countries, the Arab idea was strongly entrenched.

It is important to stress the unevenness of this process, with some parts of the Arab world coming earlier to Arabism—the term given to the early twentieth-century precursor of fully developed Arab nationalism—and others much later, with competing or complementary senses of identity stronger in some regions than in others. These alternate senses of identity, which in many cases preceded the rise of Arabism, included religious and sectarian affiliations as well as local patriotism. The attachment to religion endured, and it has provided the basis for a sustained challenge to nationalism in the Arab world in recent years. Local loyalties ultimately became the basis for the nation-state nationalism of the twenty-odd states that eventually made up the Arab world following the collapse of the Ottoman empire and the European occupations and partitions of the Middle East during the nineteenth and early twentieth centuries. This competing form of loyalty has also mounted a challenge to Arab nationalism in recent years, although in many ways it has been a less public and more subtle one.

Contrary to the mistaken impression held by many, it is not the case that Arab nationalism was or is necessarily synonymous with pan-Arabism, that is, with the idea that all Arabs should live in a single great Arab nation-state. Indeed, despite the inflated rhetoric of certain Arab nationalist parties such as the Baath (its slogan is "one Arab nation with an eternal mission"), in most cases in which Arab nationalists have had a chance to put their ideas into practice, they have not favored the idea of a single Arab nation-state. It was certainly not the practical objective envisioned by the earliest Arabists who, when they had a brief opportunity to deliberate on and implement some of their ideas after World War I, worked through a Syrian and an Iraqi congress in Damascus for the establishment

of three separate, independent Arab states east of Suez, to be linked by dynastic and other ties: one in Syria, one in Iraq, and one in the Arabian Peninsula.

Neither the charter of the Arab League nor the approaches followed by this organization since its establishment in 1944,³ nor indeed the policies of most Arab governments since they gained their independence have in practice favored the pan-Arab idea. Those regimes that at times have appeared to do so—the most notable being Egypt and Syria during the abortive union between them from 1958 until 1961, or the Syrian neo-Baath regime of the late 1960s—were the exception rather than the rule. Moreover, on closer examination, most of these regimes can be seen to have been motivated at least as much by straightforward nation-state interest as by any pan-Arab vision of a unitary state from the Atlantic to the Gulf.⁴ The same could certainly be argued in regard to the policies of the Iraqi Baath regime in power since 1968, including its invasion, occupation, and annexation of Kuwait.

On the other hand, I do not mean to discount the importance of the Arab idea in general or of this specific pan-Arab variety of it in modern Arab political discourse. Pan-Arabism has had a powerful impact on intellectual and popular currents in the Arab world, even if its effect in terms of guiding the policies of individual Arab nation-states has been very limited. The fragmentation and weakness of the Arab world in modern times have been deeply resented by Arabs, who have tended to place the blame for this situation on the Western powers, which, during the era of high imperialism, partitioned the Middle East in a manner suiting their own interests. Many Arabs have thus sought recourse in the strength they believe unity will bring. Proponents of this approach, while sometimes referring to European models, notably Germany and Italy, frequently look to the recent past in the Middle East, when most of the Arab world was part of the Ottoman empire and not yet subject to external control. More important, they idealize and romanticize a more distant past, a time over ten centuries ago when the Arab world was strong and united and stood at the pinnacle of world civilization.

The crucial point, however, missed by those who focus obsessively on pan-Arabism, is that powerful countervailing tendencies have been present since the earliest days of Arabism, before and after World War I, that have balanced this emotional drive in favor of unity. Notable among these is nation-state nationalism, based on longstanding local loyalties, which has proven at least as tenacious as pan-Arabism for most of this century, even if it does not have the legitimacy or mass appeal of that ideology.⁵ Another is the ideology of Islam, which in its

³ Robert W. Macdonald, *The League of Arab States: A Study in the Dynamics of Regional Organization* (Princeton, N.J., 1965), though outdated, is still a useful source.

⁴ For an illuminating discussion of the interaction between nation-state nationalism and pan-Arabism analyzed in terms of *raison d'état* and *raison de la nation*, see Walid Khalidi, "Thinking the Unthinkable," *Foreign Affairs*, 56 (July 1978): 695–713.

⁵ Among the best studies of the emergence of specific nation-state nationalisms in the Arab world are Muhammad Y. Muslih, *The Origins of Palestinian Nationalism* (New York, 1988); Kamal Salibi, *A House of Many Mansions: The History of Lebanon Reconsidered* (Berkeley, Calif., 1988); and Israel Gershoni and James P. Jankowski, *Egypt, Islam and the Arabs: The Search for Egyptian Nationhood, 1900–1930* (New York, 1986); see also Gershoni's *Emergence of Pan-Arabism in Egypt* (Tel Aviv, 1981). Others are discussed below.

pan-Islamic variety was strongly supported by the Ottoman state until its demise after World War I. In the current form of radical activism, Islam has proven a formidable rival to all forms of nationalism in many parts of the Arab world in recent years. It is thus grossly inaccurate to depict pan-Arabism as the only form of Arab nationalism, sweeping all before it in the Arab world, whether today or in the past.

WHEN THE EARLIEST WORKS ON ARAB NATIONALISM in English were written, beginning with George Antonius's *Arab Awakening* in 1938,⁶ and continuing through the 1960s, nation-state nationalism was less visible as a force in the Arab world than it is today, and the Islamic resurgence that has affected the region so strongly in recent years was not yet apparent to most observers. Through the 1960s, indeed, the intellectual and ideological ascendancy of Arabism in Arab politics seemed assured, and writers on the subject took this as a given. Perhaps the most useful way to classify the authors of these early works is on the basis of how skeptically they approached Arab nationalism, with one group generally positively inclined toward its subject and another highly critical, often to the point of derisiveness.

The most notable book in the first, or positive, category is Antonius's *Arab Awakening*, which, for all its many flaws, has had an enormous impact on both the scholarly and political debate over the Arab world since its publication more than half a century ago. Antonius's book is a seminal exposition of the theses of Arab nationalism, as well as a detailed account of crucial episodes in its genesis and development, much of it based on Antonius's access to both Arab and British documents not before published, interviews with key participants in events, and personal experience. Antonius, born in Egypt of Lebanese parents, was at different times in his brief career between 1921 and his premature death in 1942 an official of the British mandate administration in Palestine, an intermediary in negotiations between Britain and several Arab rulers, a negotiator on behalf of the Palestinian leadership with the British, and an official of an American foundation in the Middle East.⁷

Perhaps the best assessment of this complex book is that of the foremost historian of the modern Middle East, Albert Hourani, who stated that *The Arab*

⁶ George Antonius, *The Arab Awakening: The Story of the Arab National Movement* (London, 1938). The American edition was published the following year in Philadelphia by J. B. Lippincott.

⁷ For more on Antonius and his impact, see Albert Hourani, "The Arab Awakening Forty Years After," in *Studies in Arab History: The Antonius Lectures, 1978–87*, Derek Hopwood, ed. (London, 1990), 21–40. Hourani's was the first of the Antonius lectures, which have been delivered at St. Antony's College, Oxford, since 1978 and eleven of which are collected in this volume. For less charitable assessments of Antonius, see Martin Kramer, "Ambitious Discontent: The Demise of George Antonius," in *The Great Powers in the Middle East, 1919–1939*, Uriel Dann, ed. (New York, 1988), 405–16; and Sylvia G. Haim, "The Arab Awakening: A Source for the Historian?" *Welt des Islams*, n.s. 2 (1953). Bernard Wasserstein, *The British in Palestine*, 2d edn. (Oxford, 1991), 183–89, gives useful biographical details, as does the essay by Thomas Hodgkin, "George Antonius, Palestine and the 1930s," 77–102, in Hopwood, *Studies in Arab History*. A biography of Antonius by William Cleveland is in preparation and will complement his useful biographies of Antonius's contemporaries, Sati' al-Husri and Shakib Arslan: *The Making of an Arab Nationalist: Ottomanism and Arabism in the Life and Thought of Sati' al-Husri* (Princeton, N.J., 1971); and *Islam against the West: Shakib Arslan and the Campaign for Islamic Nationalism* (Austin, Tex., 1985).

Awakening “is a slightly uneasy combination of two different kinds of writing. It is a work of historical narrative, but also of political advocacy.”⁸ As Hourani and others have pointed out, although much of what Antonius told us is still historically valuable, many of his conclusions have been superseded by more recent research, such as his contention that Syrian Christians played a crucial role in the early development of Arab nationalism.⁹ The book nevertheless retains importance, not only because of its political impact and that of its author at the time it was published but also because scholars continue to argue for and against Antonius’s theses, which have in some measure become parameters for the field.¹⁰ In a sense, this initiator of the modern scholarly debate in the English language about Arab nationalism has continued to be a part of it during the fifty years after his death.

There is certainly great attention paid to Antonius in works that can be considered highly critical and at times even dismissive in their treatment of Arab nationalism. Prominent in this second category are the writings of Sylvia Haim and Elie Kedourie, although the same general approach permeates the work of other historians. Perhaps the most influential work in this category is the book edited by Haim titled *Arab Nationalism: An Anthology*, which has been in print since 1962.¹¹ This selection of twenty readings originating between 1901 and 1957 is notable for Haim’s analytical 70-page introduction, which presents an assessment of the readings as well as harsh portraits of many of the leading intellectual figures who influenced Arab nationalism. Jamal al-Din al-Afghani is described as having “a reputation resting on very slender evidence” (although Haim later states he “contributed to the spread of revolutionary temper and a new attitude towards politics all over the Muslim East”); the thinking on Islamic revival of al-Afghani and his disciple Muhammad ‘Abduh is called “a spurious construct of [their] imagination”; and the Syrian writer ‘Abd al-Rahman al-Kawakibi is for her “an entirely derivative thinker,” whose only original idea “he seems to have taken from somebody else.”¹²

⁸ This is Hourani’s retrospective assessment of the book in “*The Arab Awakening Forty Years After*,” 26. His article carefully analyzes what is still of value to historians in the three different parts of the book, concluding that the first two retain interest even today.

⁹ This point is best made in C. Ernest Dawn, “The Origins of Arab Nationalism,” in Rashid Khalidi, et al., eds., *The Origins of Arab Nationalism* (New York, 1991). See also the lengthy bibliographical note 1 in C. Ernest Dawn, “The Formation of Pan-Arab Ideology in the Interwar Years,” *International Journal of Middle East Studies*, 20 (February 1988): 85–86; and R. Khalidi, “Antonius, Dawn and the Rise of Arabism in Syria: Changing Views of a Changing Society,” a paper presented to a conference at the University of Illinois in November 1989, currently in preparation for publication in a volume edited by Charles Stewart.

¹⁰ Other works in the first, or positive, category include Zeine N. Zeine, *The Emergence of Arab Nationalism*, 3d edn. (Delmar, N.Y., 1973); Hazem Zaki Nuseibeh, *The Ideas of Arab Nationalism* (Ithaca, 1956); Hassan Saab, *The Arab Federalists of the Ottoman Empire* (Amsterdam, 1958); and most important among them A. L. Tibawi, *A Modern History of Syria, including Lebanon and Palestine* (London, 1969). What links these four scholarly studies with the more polemical work by Antonius (for all their disagreements with him) is their uncritical acceptance, and at times advocacy, of some of the central tenets of the ideology they describe. Saab (p. ix), for example, stated that “the urge for Arab unity is essentially an urge toward the *reconstruction of one united Arab state*” (italics in the original), thereby adopting the nationalist thesis that the early Islamic empires were a “united Arab state,” which was simply being “reconstructed” in the modern era.

¹¹ Sylvia G. Haim, *Arab Nationalism: An Anthology* (Berkeley, Calif., 1962).

¹² Haim, *Arab Nationalism*, 6, 7, 21, 27.

Haim argued in the preface to a later edition of her book that "Arab nationalism is now less a step toward unification, in spite of some outward signs in that direction, than a belief with a particular aim, namely, the defeat of the State of Israel."¹³ While Haim's skepticism regarding the trend toward unification seems well placed, and has been borne out by events, her main point is questionable. Arab nationalism has indeed been linked to opposition to Zionism since the earliest years of the two movements, but they have had a shifting relationship, as is evidenced by a variety of attempts to arrive at a settlement between them since the Faisal-Weizmann accord in 1919, and indeed even before that.¹⁴ Moreover, to reduce Arab nationalism to no more than opposition to the state of Israel, as Haim seems to be doing, is surely unjustified given the differing resonances of the ideology in different parts and in different social strata of the Arab world over nearly a century, although it is highly revealing of her attitude toward her subject.

Among the key historical questions raised by Haim, and even more forcefully by Kedourie, is the crucial and complex linkage between Arab nationalism and Islam. Haim sees a shift in recent decades toward a closer identification between the two and adds: "By becoming acceptable to Muslims, Arab nationalism is no longer merely a doctrine for the verbose and the gullible." In the less contemptuous words of Kedourie, "Islam and Arabism largely overlap; and where they do not, they are not in opposition."¹⁵ Such views are widely held, as can be seen from the argument of Bernard Lewis, in the much-reprinted monograph *The Arabs in History*, regarding the identity of Islam and nationalism for Muslim Arabs: "For Muslims the two forms of expression were never really distinguished."¹⁶

These assertions are worth considering carefully. Certainly, much evidence exists to show that many Arabs have not drawn a sharp distinction between Islam and Arabism; they were different but closely linked forms of expressions of identity made all the more important by the encroachment of the West. This attitude was particularly prevalent in the Arabian Peninsula, as well as in some parts of North Africa, such as Algeria and Libya, where national resistance to European colonialism very early took on a religious form. The idea of a near identity between Islam and Arabism is much less satisfactory, however, when applied to Egypt and the countries of the Fertile Crescent, which are the central Arab lands and the sources of the most advanced syntheses of both nationalist and Islamic revivalist doctrine, and which at the same time contain large Arab-Christian populations.

In these central Arab regions, what we may call the Haim-Kedourie thesis manifestly fails to take into account the full scope and complexity of the pre-World War I interaction between early Islamic reformers and the young

¹³ Sylvia G. Haim, ed., *Arab Nationalism: An Anthology* (Berkeley, Calif., 1976), ix.

¹⁴ For more on the earliest Arab opposition to Zionism, see Neville J. Mandel, *The Arabs and Zionism before World War I* (Berkeley, Calif., 1976); and R. Khalidi, "The Role of the Press in the Early Arab Reaction to Zionism," *Peuples méditerranéens*, 20 (juillet-septembre 1982): 105-23. On Arab-Zionist negotiations, see Neil Caplan, *Futile Diplomacy*, 2 vols. (London, 1983-86).

¹⁵ Haim, *Arab Nationalism* (1976), ix. Elie Kedourie, *Islam in the Modern World, and Other Studies* (London, 1980), 56.

¹⁶ Bernard Lewis, *The Arabs in History*, 4th edn. (London, 1966), 173.

generations of nationalists they influenced, both in Egypt and Syria,¹⁷ or the spread of secularism among the elites of this region during this century. Secularism in particular seems to have escaped the attention of writers of the critical school we are discussing, who rightly point to a widespread attachment to Islam that did tend to blur distinctions but who fail to address the clear delimitation between Islam and Arabism envisioned by the most advanced formulations of Arab nationalism, whether in the versions put forward by Sati' al-Husri in Iraq in the 1930s and 1940s, the Baath party in Syria in the 1950s, or Nasserism in Egypt in the 1960s.

Equally serious, historians of this school have failed to appreciate the tension between nationalism and Islamic activism that has been latent since the foundation of the Muslim Brotherhood in Egypt in the 1930s, flared up anew in Egypt after the 1952 revolution, and has been a permanent feature of the politics of Egypt, Syria, Lebanon, Iraq, and the Palestinians since at least 1970. Certainly since the 1950s, the brotherhood and their various radical Islamic offshoots, allies, and imitators have considered nationalism, whether in its full-blown Arab nationalist form or that of nation-state nationalism, to be an abomination, as is shown most clearly by Emmanuel Sivan in his perceptive *Radical Islam*.¹⁸ In many Arab countries, these Islamic movements have come to regard nationalist parties and forces as their main enemies (an attitude of hostility that is in many cases heartily reciprocated). This was the case for the Da'wa movement in Iraq throughout the 1980s, for the underground Islamic movements in Syria in the late 1970s and early 1980s, for the Amal movement in Lebanon before the Israeli invasion in 1982, and for the Islamic movements in the occupied West Bank and Gaza Strip until the *intifada* began in 1987, and perhaps even afterwards. The Haim-Kedourie thesis prepares us for none of this.

Two other weaknesses of this approach must be touched on, at least in passing. The first is a tendency to reduce ideology to the pettiest of personal motivations on the part of its formulators. Whether with regard to al-Afghani, Kawakibi, or Antonius, we find in Haim and Kedourie a tone of contempt for personal failings described in detail, then closely linked to the ideas of these individuals.¹⁹ While such forays into psychohistory undoubtedly have a place in assessing ideas and ideologies, they loom far too large in the work of these authors. Like other ideas and ideologies, Arab nationalism deserves to be studied for itself and in terms of the social and political forces that espouse it, as well as in terms of individuals and their personal failings.

The second is the persistent argument, found for example in Kedourie, that

¹⁷ The recent work of David Dean Commins, notably his excellent book *Islamic Reform: Politics and Social Change in Late Ottoman Syria* (New York, 1990), and his article, "Religious Reformers and Arabists in Damascus," *International Journal of Middle East Studies*, 18 (November 1986): 405–25, help elucidate this vital relationship and the way in which Islamic reformist thought influenced Arabism, as does Joseph H. Escovitz's article, "'He was the Muhammad 'Abduh if Syria': A Study of Tahir al-Jaza'iri and his Influence," *International Journal of Middle East Studies*, 18 (August 1986): 293–310.

¹⁸ Emmanuel Sivan, *Radical Islam: Medieval Theology and Modern Politics* (New Haven, Conn., 1985).

¹⁹ These figures are dealt with in Elie Kedourie, *Afghani and 'Abduh: An Essay on Religious Unbelief and Political Activism in Modern Islam* (London, 1966); Sylvia Haim, "Alfieri and Kawakibi," *Oriente moderno*, 34 (1954); Sylvia Haim, "Blunt and Kawakibi," *Oriente moderno*, 35 (1955); and Kedourie, *Nationalism in Asia and Africa*, 85–87.

Arab nationalism is in some measure a bastard creation of its British official sponsors during the first half of this century.²⁰ Kedourie was certainly correct in arguing that Britain originally adopted Arab nationalism as a weapon against the Ottoman empire—indeed, I have argued elsewhere that this process began even before the World War I agreements with Sharif Husayn, which most historians note as its starting point²¹—and continued to favor and support client elites that espoused Arab nationalism until well after World War II. But this approach ignores the rest of these Arab societies at the level below the few leading figures with whom the British dealt and the way in which Arabism developed, not just at the elite level but elsewhere in society. In other words, Arabism existed as an ideology and a popular political force independent of all the twisted relationships between imperial Britain and its Arab clients that Kedourie chronicles, and it cannot be understood only in terms of these relationships, important though they were.

THE QUESTIONS RAISED by these early works on Arab nationalism—about the relationship between Arabism and Islam, and the influence of the West on the genesis of Arab nationalism—are considered further in some of the more recent scholarly work on this topic. Other crucial issues treated in recent literature include the uneasy relationship between Arab nationalism and the nation-state nationalism of the different Arab states that have emerged in the modern period and the social basis of Arabism and its ideological rivals. These issues receive a magisterial treatment from Albert Hourani in *A History of the Arab Peoples*, a new and very well-received synthesis of the whole sweep of Arab history, whose most important contribution is a careful consideration of the problems of the modern period, of which Hourani is the acknowledged master.²² After a career spanning more than forty years of teaching and research, he looks carefully, in the lengthy segment of the book on the Arab world since the beginning of the nineteenth century, at all four of the historical issues I have just underlined, in particular the relationship between Islam and Arabism and that between Arabism and the outside world, which have always preoccupied him, as they preoccupied Antonius and Kedourie.

Besides Albert Hourani, the historian who most thoroughly addresses these questions is C. Ernest Dawn, notably in *From Ottomanism to Arabism*.²³ In this collection, and in later writings, Dawn has persuasively delineated some of the

²⁰ Kedourie has put forward this argument repeatedly in his many writings, perhaps most notably in his first book, *England and the Middle East: The Destruction of the Ottoman Empire, 1914–1921* (London, 1956); and in the title essay in *The Chatham House Version, and Other Middle-Eastern Studies* (London, 1970), 351–94.

²¹ Rashid Khalidi, *British Policy towards Syria and Palestine, 1906–1914: A Study of the Antecedents of the Hussein-McMahon Correspondence, the Sykes-Picot Agreement, and the Balfour Declaration* (London, 1980).

²² Albert Hourani, *A History of the Arab Peoples* (Cambridge, Mass., 1991). The book reached number 3 on the *New York Times* best-seller list after its publication, a remarkable feat for such a scholarly work.

²³ C. Ernest Dawn, *From Ottomanism to Arabism: Essays on the Origins of Arab Nationalism* (Urbana, Ill., 1973); this volume brings together essays most of which were originally published between 1957 and 1962; see also the two more recent essays by Dawn cited above in n. 9.

different ways in which Arabism has related to Islam in the Arab world. He has shown with relation to Syria, the Hijaz, and Egypt in particular that a more complex interaction took place than that depicted by either Antonius or Haim and Kedourie, critiquing the views of the former and refining significantly those of the latter. Moreover, in describing the early relations between Arab elites (notably the Hashemites) and the British, Dawn has both followed lines laid down by Antonius and Kedourie and developed his own insightful analysis, showing the Hashemites to be dynasts for whom Arab nationalism was a useful instrument in some circumstances and of little relevance in others. This analysis has been largely borne out in monographs on the Hijaz and Jordan by William Ochsenwald and Mary Wilson respectively, and in a book on Iraq by Hanna Batatu.²⁴

Dawn's interpretation of the social basis of Arabism in Syria—that it was essentially the vehicle of a minority faction of the traditional upper-class elite until after World War I—has been very influential, although it has been challenged in a number of respects.²⁵ It nevertheless remains the first serious attempt to elucidate an important aspect of Arab nationalism and one that has not yet received the attention it deserves. It addresses the relationship between ideas and society, specifically the way in which changes in the dominant ideology in the Arab world, notably that from Ottomanism to Arabism in the first two decades of this century, relate to simultaneous transformations in Arab society.

The paucity of work on this subject is attributable in part to the relative lack of scholarly attention to the social history of the Arab world, with a few important exceptions, such as those sections relating to Arab society in Haim Gerber's *Social Origins of the Modern Middle East*, in volumes such as *The Rural Middle East: Peasant Lives and Modes of Production*, edited by Kathy Glavanis and Pandeli Glavanis, and in the pioneering work of the late Gabriel Baer.²⁶ The situation is somewhat better in the field of economic history, where the extensive writings of Charles Issawi and Roger Owen have illuminated both the internal development of the Middle Eastern economies and their relations with the expanding world economy in the modern era.²⁷ Unfortunately, there has as yet been little impact from research in

²⁴ William Ochsenwald, *Religion, Society, and the State in Arabia: The Hijaz under Ottoman Control, 1840–1908* (Columbus, Ohio, 1984); Mary C. Wilson, *King Abdullah, Britain and the Making of Jordan* (Cambridge, 1987); see also their respective essays in R. Khalidi, *Origins of Arab Nationalism*, "Ironie Origins: Arab Nationalism in the Hijaz, 1882–1914," and "The Hashemites, the Arab Revolt and Arab Nationalism"; Hanna Batatu, *The Old Social Classes and the Revolutionary Movements of Iraq: A Study of Iraq's Old Landed and Commercial Classes and of Its Communists, Ba'ithists, and Free Officers* (Princeton, N.J., 1978.)

²⁵ Dawn's thesis was put forward in its original form in his essay, "The Rise of Arabism in Syria," in *From Ottomanism to Arabism*, and expanded in his two articles cited in n. 9. For a critique of them, see R. Khalidi, "Ottomanism and Arabism in Syria before 1914: A Reassessment," in R. Khalidi, *Origins of Arab Nationalism*; and "Society and Ideology in Late Ottoman Syria: Class, Education, Profession and Confession," in John Spagnolo, ed., *Taking the Long View on the Modern Middle East: Essays in Honour of Albert Hourani* (Oxford, 1991).

²⁶ Haim Gerber, *The Social Origins of the Modern Middle East* (Boulder, Colo., 1987); Kathy Glavanis and Pandeli Glavanis, eds., *The Rural Middle East: Peasant Lives and Modes of Production* (London, 1990). Gabriel Baer's most important works of social history include *Studies in the Social History of Modern Egypt* (Chicago, 1969); and *Fellah and Townsman in the Middle East: Studies in Social History* (London, 1982). See also Edmund Burke III and Ira M. Lapidus, eds., *Islam, Politics, and Social Movements* (Berkeley, Calif., 1988); and Tarif Khalidi, ed., *Land Tenure and Social Transformation in the Middle East* (Beirut, 1984).

²⁷ Charles Issawi's writings on Egypt's economic history alone are impressive, encompassing three

these areas on our understanding of ideology in the modern Arab world, whether Ottomanism, Arabism, the different nation-state nationalisms, or Islamic radicalism.

Similarly, much work remains to be done to link our growing understanding of the increasingly important nationalisms of the individual Arab states with what we know of the transnational movements such as Arabism and Islamic radicalism, which compete for the loyalty of their citizens. We are in a better position to understand these individual nationalisms today than a few decades ago, because the work being done on them represents some of the most interesting research in the field of modern Middle Eastern history. Philip Khoury's two books on Syria, Kamal Salibi's brilliant latest work on Lebanon, Yehoshua Porath's two books on Palestinian nationalism, as well as that of Muhammad Muslih on the same subject, Gershoni and Jankowski's volume on Egypt, together with other monographs, are evidence that much outstanding research is being done on the modern history of the nationalisms arising in individual Arab countries.²⁸

Missing is a synthesis that will tie together and explain the common factors between these different national movements, provide an analysis of the meaning of their emergence, and explore the delicate balance between the transnational, the national, and the subnational in an Arab world whose very form and shape are still indefinite seventy years after the post-World War I settlement. This settlement fixed those of the region's current boundaries that had not already been delimited by European powers before the war. Although few of these boundaries have changed since 1921, the settlement has clearly left the Arab peoples with a heavy, unresolved legacy of problems of legitimacy, identity, and relations with the outside world.²⁹ Future work in this field needs to address some of these problems, incorporating both worthwhile methodological and critical approaches from outside the field of Middle Eastern history and the most valuable conclusions of the works discussed above.

major works, to which must be added his valuable documentary economic histories of the Middle East, Iran, Turkey, and the Fertile Crescent, as well as his analytical *An Economic History of the Middle East and North Africa* (New York, 1982). Roger Owen is the author of *The Middle East in the World Economy, 1800–1914* (London, 1981), which is the best treatment of its topic; *Cotton and the Egyptian Economy* (London, 1969); and several other valuable works treating the relations between politics, economics, and society in the Middle East.

²⁸ Philip S. Khoury, *Urban Notables and Arab Nationalism: The Politics of Damascus 1860–1920* (Cambridge, 1983); and *Syria and the French Mandate: The Politics of Arab Nationalism, 1920–1945* (Princeton, N.J., 1987); Salibi, *House of Many Mansions*; Yehoshua Porath, *The Emergence of the Palestinian-Arab National Movement, 1918–1929* (London, 1974); and *The Palestinian Arab National Movement: From Riots to Rebellion, 1929–1939* (London, 1977), as well as his *In Search of Arab Unity, 1930–1945* (London, 1986); Muslih, *Origins of Palestinian Nationalism*; Gershoni and Jankowski, *Egypt, Islam and the Arabs*. See also Ami Ayalon, *Language and Change in the Arab Middle East: The Evolution of Modern Political Discourse* (New York, 1987).

²⁹ For a thoughtful treatment of these issues, see the final two chapters of Hourani, *History of the Arab Peoples*. On the post-World War I settlement, see David Fromkin, *A Peace to End All Peace: Creating the Modern Middle East, 1914–1922* (New York, 1989), which treats primarily the European decision-making process, on the basis of Western sources, since, in Fromkin's words, "in the 1914–1922 period, Europeans and Americans were the only ones seated around the table when the decisions were made"; p. 17. It is thus one of the best examples of the old-style diplomatic history, which ignores the natives as unimportant.

Continuity and Change in Syrian Political Life: The Nineteenth and Twentieth Centuries

PHILIP S. KHOURY

HISTORIANS HAVE DEMONSTRATED a penchant for tracking the forces of change stemming from the political and socioeconomic upheavals that have punctuated the history of the Middle East in the past two hundred years. The effects of the Ottoman reformation (or Tanzimat), the European industrial revolution, World War I, and the collapse of four centuries of Ottoman rule followed by the imposition of European control over much of the Arab Middle East have steered historians in this direction. As a consequence, they have shown considerably less interest in looking for elements of continuity and stability among the many transformations experienced by the region. This benign neglect holds true for historians of the Middle East regardless of the methodologies and frameworks of analysis they apply to their subjects. Indeed, the social and economic historian is no different from today's less-fashionable political historian and the liberal historian no different from the conservative. The study of urban political culture in Syria in the nineteenth and twentieth centuries is a case in point.

I will argue that political culture in Syria did not change abruptly with the break-up of the Ottoman empire and the imposition of European rule at the end of World War I. Rather, the exercise of political power followed what can be called the Ottoman model for nearly four decades after the demise of the empire. In order to support this contention, three periods of modern Syrian history need to be examined. In the first period, from the mid-nineteenth century until the early twentieth, a political culture in the towns of Syria arose that was intimately tied to the emergence of a single political elite. During this time, the urban elites developed a distinct social character and political role. The second period begins with the collapse of the Ottoman empire and the introduction of French rule and ends with World War II and France's abandonment of Syria. A remarkable degree of continuity in Syrian urban political culture and in the character of the Syrian elite's political role distinguished this era, despite the major upheavals that foreshadowed and characterized the interwar years. The third period corresponds to the early years of Syrian independence. Only then did Syrian political culture begin to assume radically new forms and dimensions, but even this process took nearly two decades to unfold.

This essay is adapted, with permission, from the author's article "Syrian Political Culture: A Historical Perspective," in Richard T. Antoun and Donald Quataert, eds., *Syria: Society, Culture, and Polity* (Albany, N.Y., 1991).

ALTHOUGH MY UNITS OF ANALYSIS in this essay are the major interior towns of what eventually came to be known as the Syrian Arab Republic, a familiar political culture existed throughout the eastern Arab provinces of the Ottoman empire, in Palestine and along the coast of what is today Lebanon. This political culture was distinctly urban and was shaped by a mixture of Ottoman administrative practices, local Arab traditions, and European intellectual and material influences. In Syria's case, independent rural politics on anything more than a local scale began in the post-World War II era, when the rural peoples, especially the compact religious minorities on the Syrian periphery—'Alawis in the rugged mountain districts of northwestern Syria and Druzes in the inhospitable hills southeast of Damascus—began their entrance onto the wider stage of national politics.¹

The political configuration of the larger Syrian towns during the Ottoman period suggests two areas of political power: one was external in the guise of the Ottoman state, with its imperial capital at Istanbul, and included the provincial governors and imperial troops; the other was internal, filled by local groups possessing varying degrees of independent social and political influence and acting as intermediaries between the state and the urban populace. The Turkish-speaking governors sent out from Istanbul often were not familiar with the Arabic language and local customs and usually did not have sufficient military backing to exercise direct control in the provinces. Thus they had to rely on those local forces with independent influence in society. This Ottoman dependence on local hierarchies of power allowed a particular type of politics to emerge in the Arab provinces, which Albert Hourani has called a "politics of notables."² In some important ways, this mode of urban politics was operative not only in Ottoman times,³ but also before, during the medieval period, and after, in the interwar

¹ Raymond A. Hinnebusch has written that "for the great mass of the peasantry, government remained a threatening power to be evaded or submitted to, not a system on which demands could be made or support given. Until peasants produced their own leadership from educated sons of the village, rural politics was not to take on a significant sustained class dimension, and national politics remained an urban game largely isolated from village needs and wishes"; *Authoritarian Power and State Formation in Ba'thist Syria: Army, Party, and Peasant* (Boulder, Colo., 1990), 45. There were complex factors that inhibited the development of independent rural politics before World War II, among them the divisive geography and precarious ecology of agrarian Syria, the continuous pull of primordial loyalties to tribe, clan, village, sect and ethnic group, the concentration of agricultural land in the hands of a small number of powerful families resident in the towns, and the consequent organization of politics along clientelist lines. Although socially and economically diverse, the bulk of peasant society found itself squeezed and ultimately dominated by Syria's urban elites from one side and by nomadic pastoralists from the other. On the structure of rural society in Syria and the character of peasant movements, see Jacques Weulersse, *Paysans de Syrie et du Proche-Orient* (Paris, 1946); 'Abdullah Hanna, *al-Qadiyya al-zira'iyya wa'l-harakat al-fallahiyya fi suriyya wa lubnan (1820–1920)*, vol. 1. (Beirut, 1975); and *al-Qadiyya al-zira'iyya wa'l-harakat al-fallahiyya fi suriyya wa lubnan (1920–1945)*, vol. 2. (Beirut, 1978); Jean Hannover, "Le Monde rural avant les réformes," in André Raymond, ed., *La Syrie d'aujourd'hui* (Paris, 1980), 273–95.

² Albert Hourani, "Ottoman Reform and the Politics of Notables," in William R. Polk and Richard L. Chambers, eds., *Beginnings of Modernization in the Middle East: The Nineteenth Century* (Chicago, 1968), 41–68. The year before Hourani's seminal article was published, Ira M. Lapidus produced *Muslim Cities in the Later Middle Ages* (Cambridge, Mass., 1967), which has also offered historians important and lasting instruction on how to comprehend urban politics at the macro and micro levels in the eastern Arab-Islamic lands. Written independently of one another and for different historical periods, both studies posit that local politics in the major towns and in the countryside around them were dominated by groups of notables in competition with one another for power and influence.

³ See Abdul-Karim Rafeq, *The Province of Damascus, 1723–1783* (Beirut, 1966); Moshe Ma'oz, *Ottoman Reform in Syria and Palestine, 1840–1861: The Impact of the Tanzimat on Politics and Society*

years of the twentieth century, and even in the years immediately following World War II.⁴

Historians have used versions of the notables paradigm⁵ to study the political configuration of the major towns, the rival factions that dominated them, the relations between notables and foreign rulers, and the role of notables (*a'yan*) in provincial administration, land control, production and trade, and religious organizations, including the Sufi (mystical) orders.⁶ The notables paradigm has been especially valuable in studying the emergence of Arab nationalism in Syria,

(London, 1968); Max L. Gross, "Ottoman Rule in the Province of Damascus, 1860–1909" (Ph.D. dissertation, Georgetown University, 1979); Karl K. Barbir, *Ottoman Rule in Damascus, 1708–1758* (Princeton, N.J., 1980); Margaret Lee Meriwether, "The Notable Families of Aleppo, 1770–1830: Networks and Social Structure" (Ph.D. dissertation, University of Pennsylvania, 1981); Philip S. Khoury, *Urban Notables and Arab Nationalism: The Politics of Damascus, 1860–1920* (Cambridge, 1983); see also Ruth Roded, "Ottoman Service as a Vehicle for the Rise of New Upstarts among the Urban Elite Families of Syria in the Last Decades of Ottoman Rule," in Gabriel R. Warburg and Gad G. Gilbar, eds., *Studies in Islamic History: Contributions in Memory of Gabriel Baer* (Haifa, 1984); Roded, "The Syrian Urban Notables: Elite, Estates, Class?" *Asian and African Studies*, 20 (1986): 375–84; and Roded, "Social Patterns among the Urban Elite of Syria during the Late Ottoman Period, 1876–1918," in David Kushner, ed., *Palestine in the Late Ottoman Period: Political, Social and Economic Transformation* (Jerusalem, 1986); Linda Schatkowski Schilcher, *Families in Politics: Damascene Factions and Estates of the Eighteenth and Nineteenth Centuries* (Stuttgart, 1985); Abdul-Rahim Abu-Husayn, *Provincial Leaderships in Syria, 1575–1650* (Beirut, 1985); Haim Gerber, *Ottoman Rule in Jerusalem, 1890–1914* (Berlin, 1985); Abraham Marcus, *The Middle East on the Eve of Modernity: Aleppo in the Eighteenth Century* (New York, 1989).

⁴ For the medieval period, see Lapidus, *Muslim Cities in the Later Middle Ages*; Boaz Shoshan, "The 'Politics of Notables' in Medieval Islam," *Asian and African Studies*, 20 (1986); and R. Stephen Humphreys, "Politics and Architectural Patronage in Ayyubid Damascus, 1193–1260," in C. E. Bosworth and A. L. Udovitch, eds., *The Islamic World from Classical to Modern Times: Essays in Honor of Bernard Lewis* (Princeton, N.J., 1988). On the interwar years, see Philip S. Khoury, *Syria and the French Mandate: The Politics of Arab Nationalism, 1920–1945* (Princeton, 1987). For the postwar period, see Patrick Seale, *The Struggle for Syria: A Study of Post-War Arab Politics, 1945–1958* (London, 1965); and C. Ernest Dawn, "Ottoman Affinities of 20th Century Regimes in Syria," in Kushner, *Palestine in the Late Ottoman Period*.

⁵ See Philip S. Khoury, "The Urban Notables Paradigm Revisited," *Revue du monde musulman et de la Méditerranée*, 55–56 (1990–91): 215–28. Historians working with the notables paradigm for Ottoman Syria have relied on local Arabic chronicles, biographical dictionaries, and, most important, provincial Islamic law (*shari'a*) court registers. These have been supplemented by published Ottoman yearbooks (*salname*) and a variety of European consular archives and travelogues. Few, however, have made significant use of the Ottoman imperial archives in Istanbul to study the notables of geographic Syria; therefore, the view from the imperial capital remains largely a mystery. Two instructive articles on the uses of the law court registers are Abdul-Karim Rafeq, "The Law-Court Registers and Their Importance for a Socio-Economic and Urban Study of Ottoman Syria," in Dominique Chevallier, ed., *L'Espace social de la ville arabe* (Paris, 1979), 51–58; James A. Reilly, "Shari'a Court Registers and Land Tenure around Nineteenth-Century Damascus," *Middle East Studies Association Bulletin*, 21 (1987): 155–69.

⁶ See Schatkowski Schilcher, *Families in Politics*; Roded, "Ottoman Service as a Vehicle for the Rise of New Upstarts"; Ramez G. Tomeh, "Landowners and Political Power in Damascus, 1858–1958" (Master's thesis, American University of Beirut, 1977); Bu 'Ali Yasin, *Hikayat al-ard wa'l-fallah al-suri, 1858–1979* (Beirut, 1979); Wajih al-Kawtharani, *Bilad al-Sham, al-sukkan, al-iktisad wa'l-siyasa al-faransiyya fi matla' al-qarn al-ishrin: qira'a fi'l wilayat* (Beirut, 1980); Abdul-Karim Rafeq, "Economic Relations between Damascus and the Dependent Countryside," in A. L. Udovitch, ed., *The Islamic Middle East, 700–1900: Studies in Economic and Social History* (Princeton, N.J., 1981); Antoine Abdel-Nour, *Introduction à l'histoire urbaine de la Syrie ottomane (XVI^e–XVIII^e siècle)* (Beirut, 1982); James A. Reilly, "Origins of Peripheral Capitalism in the Damascus Region, 1830–1914" (Ph.D. dissertation, Georgetown University, 1987); and Reilly, "Status Groups and Propertyholding in the Damascus Hinterland, 1828–1880," *International Journal of Middle East Studies*, 21 (1989): 517–39; Bruce Masters, *The Origins of Western Economic Dominance in the Middle East: Mercantilism and the Islamic Economy in Aleppo, 1600–1750* (New York, 1988).

Lebanon, and Palestine and the most important political and intellectual leaders of that movement in its formative stages.⁷

To become and remain viable political actors, notables relied on a variety of vertical linkages both to dependent groups in the wider society below them and to their foreign governors or rulers above them, whether in the provincial capitals of Damascus and Aleppo or in Istanbul. To enhance their strength, notables also formed horizontal alliances, often of a temporary sort, with other notables. Factions of notables competed with one another for influence in society and with their foreign rulers. The material base of the notables' power and influence was frequently rooted in control of land or the land tax, urban real estate, local handicrafts, regional and long-distance trade, and pious trusts (*awqaf*). Some notables belonged to the ranking families of the religious establishment, while others derived power from their control of local military organizations.

If there is one characteristic that helped define the notables, it was their ability to act as intermediaries between government and local society. Political survival required a delicate balancing act. Notables could not appear to oppose the interests of the government, because they risked being deprived of their access to the ruler; nor could they jeopardize the interests of their local clientele, because they risked losing their independent influence and thus their usefulness to the ruler. Access and patronage were the code words of the politics of notables.

It was usual for local notables to defend the social and political order by supporting the government. But there were occasions on which they led movements against the government by mobilizing those popular forces from which they derived their independent influence. Such occasions arose when a particularly strong governor sought to dissolve the compact between himself and the local notables or when a weak governor could no longer maintain the stability that ensured the prosperity of the local leadership. Rarely, however, did urban notables aim to overthrow the system of rule. Rather, their actions were typically intended to preserve the delicate balance between government and society. The important point to emphasize is that the various political and social movements, revolts, and urban insurrections that occurred in Syria in the nineteenth century were more often restorative than revolutionary movements.⁸

⁷ See C. Ernest Dawn, *From Ottomanism to Arabism: Essays on the Origins of Arab Nationalism* (Urbana, 1973); Yehoshua Porath, *The Emergence of the Palestinian-Arab National Movement, 1918–1929* (London, 1974); Ann Mosely Lesch, *Arab Politics in Palestine, 1917–1939: The Frustration of a Nationalist Movement* (Ithaca, N.Y., 1979); Khoury, *Urban Notables and Arab Nationalism*; Muhammad Y. Muslih, *The Origins of Palestinian Nationalism* (New York, 1988); William L. Cleveland, *The Making of an Arab Nationalist: Ottomanism and Arabism in the Life and Thought of Sati' al-Husri* (Princeton, N.J., 1971); and *Islam against the West: Shakib Arslan and the Campaign for Islamic Nationalism* (Austin, Tex., 1985); Mary C. Wilson, *King Abdullah, Britain and the Making of Jordan* (Cambridge, 1987); and "The Hashemites, the Arab Revolt, and Arab Nationalism," in Rashid Khalidi, et al., eds., *The Origins of Arab Nationalism* (New York, 1991); Philip Mattar, *The Mufti of Jerusalem: Al-Hajj Amin al-Husayni and the Palestinian National Movement* (New York, 1988).

⁸ Hourani has made the general argument that restoration, not revolution, tended to be the norm at the higher levels of politics in Ottoman times and during the interwar years in Syria. Albert Hourani, "Revolution in the Arab Middle East," in P. J. Vatikiotis, ed., *Revolution in the Middle East, and Other Case Studies* (London, 1972), 65–72. Revolutionary movements, as such, were rare occurrences in this period, whether in the towns or countryside. But compare 'Abdullah Hanna, *al-Qadiyya al-zira'iyya wa'l-harakat al-fallahiyya fi suriyya wa lubnan (1820–1920)*; and *Harakat al-ammah al-dimashqiyya fi'l-qarnayn al-thamin 'ashar wa'l-tasi 'ashar: Numudhaj li-hayat al-mudun fi zill al-'iqtaiyya*

Before the second half of the nineteenth century, urban leaders in Syria may have had similar aims, but they did not form a single sociological type; their social origins and the foundations of their wealth varied, as did their geographic distribution throughout the towns. Thus the sources of their independent influence were also varied. For purposes of discussion, the urban notables can be grouped into three kinds of leaders, differentiated from one another by their status group, the nature of their material resource bases, and their physical location in the towns. There were the leaders of the great Muslim religious families, including members of the *ulama* (those learned in the religious sciences) and the *ashraf* (claimants to descent from the Prophet Muhammad), who were the guardians of urban civilization and Islamic high culture;⁹ secular dignitaries, a category composed of merchants and tax farmers; and the *aghass* (commanders or chiefs) of the local Janissary garrisons.

The leading religious families were mainly concentrated in the ancient quarters of the old walled city, near the cathedral mosque and central bazaar, where they had strong ties to the traditional commercial activities of production and trade. By the later Middle Ages, the great religious families were often among the most prominent commercial families; hence their wealth came not only from their control of pious trusts but also from their control of production and trade, and, by the eighteenth century, from hereditary tax farms (*malikane*) around the towns.¹⁰

The secular dignitaries were less easily differentiated from the *ulama* and the *ashraf* in terms of where they resided in the towns, and they were also a much

al-sharqiyya, vol. 1 (Beirut, 1985). Studies of political, socioeconomic, and sectarian unrest in nineteenth-century Syria include Ma'oz, *Ottoman Reform*, 131–40; Schatkowski Schilcher, *Families in Politics*, 87–106; Linda Schatkowski Schilcher, "The Hauran Conflicts of the 1860s: A Chapter in the Rural History of Modern Syria," *International Journal of Middle East Studies*, 13 (1981): 159–79; and "Violent Confrontations in Rural Syria of the 1880's–1890's," in John Waterbury and Farhad Kazemi, eds., *Peasants and Politics in the Modern Middle East* (Miami, 1991); Herbert L. Bodman, *Political Factions in Aleppo, 1760–1826* (Chapel Hill, N.C., 1963); Bruce Masters, "The 1850 Events in Aleppo: An Aftershock of Syria's Incorporation into the Capitalist World System," *International Journal of Middle East Studies*, 22 (1990): 3–30. For the interwar period, see Zafer Kassimiy, "La Participation des classes populaires aux mouvements nationaux d'indépendance aux XIX^e et XX^e siècles: Syrie," in Commission Internationale d'Histoire des Mouvements Sociaux et des Structures Sociales, ed., *Mouvements nationaux d'indépendance et classes populaires aux XIX^e et XX^e siècles en Occident et en Orient* (Paris, 1971); David Buchanan McDowall, "The Druze Revolt, 1925–27, and Its Background in the Late Ottoman Period" (B. Litt. dissertation, University of Oxford, 1972); 'Abdullah Hanna, *al-Haraka al-'ummaliyya fi suriyya wa lubnan 1900–1945* (Damascus, 1973); and *al-Qadiyya al-zira'iyya wa'l-harakat al-fallahiyya fi suriyya wa lubnan (1920–1945)*, vol. 2 (Beirut, 1978); Edmond Rabbath, "L'Insurrection syrienne de 1925–1927," *Revue historique*, 542 (avril–juin 1982): 405–47; Philip S. Khoury, "A Reinterpretation of the Origins and Aims of the Great Syrian Revolt, 1925–1927," in George N. Atiyeh and Ibrahim M. Oweiss, eds., *Arab Civilization: Challenges and Responses* (Albany, N.Y., 1988), 241–71; Abdul-Karim Rafeq, "Gesellschaft, Wirtschaft und politische Macht in Syrien 1918–1925," in Linda Schatkowski Schilcher and Claus Scharf, eds., *Der Nahe Osten in der Zwischenkriegszeit 1919–1939: Die Interdependenz von Politik, Wirtschaft und Ideologie* (Stuttgart, 1989), 440–81; Peter Sluglett, "Urban Dissidence in Mandatory Syria: Aleppo 1918–1936," in Kenneth Brown, et al., *Etat, ville et mouvements sociaux au Maghreb et au Moyen-Orient* (Paris, 1989), 301–16.

⁹ Evidence provided by Schatkowski Schilcher (*Families in Politics*) and Roded ("Syrian Urban Notables") indicates that the *ulama* and the *ashraf* each had their own identifiable status and, it would seem, ties to different economic activities in the towns and their hinterland.

¹⁰ Rafeq, *Province of Damascus*; Barbir, *Ottoman Rule in Damascus*; Khoury, *Urban Notables and Arab Nationalism*; Schilcher Schatkowski, *Families in Politics*.

more amorphous group whose influence depended almost exclusively on wealth, which, as Ruth Roded has suggested, was a "precarious base for prolonged prominence."¹¹ Unlike the other two status groups, they enjoyed neither inherited prestige nor independent military might.

The *aghas* commanded the local Janissary garrisons, which had permanently lodged on the outskirts of the towns by the eighteenth century. By contrast to the ulama, *ashraf*, and secular dignitaries, the *aghas* and their supporters resided outside the town walls in what amounted to suburbs. The Midan quarter to the south of the ancient town of Damascus and Bab al-Nayrab on the south-east of Aleppo were two such suburbs that supported local Janissary garrisons. They were far less homogeneous than the inner city quarters and were filled with a variety of socially marginal in-migrants—uprooted peasants and semi-sedentarized tribes who visited in the winter season, a variety of non-Arab ethnic groups, and the undifferentiated poor, who were akin to Louis Chevalier's *classes dangereuses*. None of these groups was especially welcome inside the town walls.¹²

The *aghas* derived power in at least two ways: by protecting the marginal and disenfranchised people in their quarters and integrating them into their paramilitary organizations, and by dominating the grain and livestock trade that sustained urban life. *Aghas* could even hold cities ransom by controlling the supply of grain to the towns or by fixing grain prices. In such circumstances, there was bound to be tension between the town center and the suburbs.

The *aghas* were both feared and despised by the more cosmopolitan religious establishment, which considered them socially repugnant and their power dangerous. The ulama and *ashraf* often found themselves in a conflictual relationship with the *aghas*, although they might occasionally join forces against the Ottoman governor, especially if he was unusually ruthless or weak. The religious and mercantile leaders in the city center were more committed than the *aghas* to upholding the status quo, for they were the groups with a true stake in urban society. Because they had the most to lose, urban revolt was not their preferred mechanism of correcting the balance of power in the towns when that balance was destabilized, as it so often was in the eighteenth and nineteenth centuries. They employed more subtle kinds of pressures to achieve their ends.

It would be incorrect to suggest that before the mid-nineteenth century the notables (and aspiring notables) constituted a "class" in the sense of an economic and social formation that can be defined with respect to property or, more precisely, by its relations to the means of production and to the social position of its constituents. Classes may have existed in the Syrian towns, but "classes" are not meaningful units of analysis in this period; their lifespans were too short because their relations to the means of production and especially to property were

¹¹ Roded, "Syrian Urban Notables," 180.

¹² See Abdul-Karim Rafeq, "The Social and Economic Structure of Bab al-Musalla (al-Midan), Damascus, 1825–1875," in George N. Atiyeh and Ibrahim M. Oweiss, eds., *Arab Civilization: Challenges and Responses* (Albany, N.Y., 1988), 272–311; André Raymond, *Grandes villes arabes à l'époque ottomane* (Paris, 1985); Louis Chevalier, *Classes laborieuses et classes dangereuses à Paris pendant la première moitié du XIX^e siècle* (Paris, 1958).

generally unstable.¹³ It is more appropriate, therefore, to refer to the different groups of notables as "status groups" before mid-century.¹⁴

Something happened in the second half of the nineteenth century that encouraged these rival urban status groups with independent social and political influence to merge. The elite formed by this merger, and the wider class that supported it, would dominate Syrian political life virtually unchallenged until World War II. The gradual integration of Syria into a European-dominated economy in the nineteenth century and the Ottoman reformation or Tanzimat, which marched hand-in-hand with European expansion into the Middle East, encouraged this merger.¹⁵ One corollary of the Tanzimat, that is, of the increased centralization of Ottoman authority in the provinces of the far-flung empire, was the growth and modernization of the state bureaucracy, which was rationalized and secularized. Another corollary was the modernization of the military. The new Ottoman army may have done poorly on the battlefield against the European powers, but it brought increased control over the Arab provinces after mid-century.

For the religious establishment and the military chiefs in the Syrian towns to survive, they had to adapt to the changing political climate. They could no longer move as freely as they had between the state and society. Istanbul still needed local intermediaries but only those willing to identify themselves with the reinvigorated Ottoman state and its new policies. To achieve and retain local political influence, a notable now had to move into the modernized state institutions being set up in the provinces. Never before had notables in the towns been obliged to identify their interests so closely with the state or to distance themselves so far from their bases in local society.¹⁶

BUT LOCAL POWER WAS NOT SOLELY A FUNCTION OF acquiring high government office in the provinces. It also became a function of acquiring and retaining property on a large scale. After the mid-nineteenth century, there developed a greater interest in land as a principal source of wealth. This interest had to do with the expansion of the frontiers of cultivation in Syria, the settlement of the tribes on these lands, and, to a certain extent, the commercialization of agriculture.¹⁷ At the same time, the European industrial revolution had caused a gradual deterioration of many traditional Syrian industries in the towns and of the regional trade in local manufactures. Wealthy merchants and other owners of capital

¹³ See Khoury, *Urban Notables and Arab Nationalism*, 3–4.

¹⁴ See Reilly, "Status Groups and Propertyholding," 517–39. Shatkowski Schilcher prefers to divide the notables into "estates," of which she has identified seven for Damascus in the nineteenth century; see *Families in Politics*, 107–32.

¹⁵ For the European impact on the economies of the Middle East and, in particular, on Syria, see Roger Owen, *The Middle East in the World Economy, 1800–1914* (London, 1981), 57–82, 153–79, 244–72. On the impact of the Tanzimat, see Ma'oz, *Ottoman Reform*; 'Abd al-'Aziz Muhammad 'Awad, *al-Idara al-'uthmaniyya fi wilaya suriyya, 1864–1914* (Cairo, 1969).

¹⁶ See Khoury, *Urban Notables and Arab Nationalism*, chap. 2.

¹⁷ On the settlement of tribes, see Norman N. Lewis, *Nomads and Settlers in Syria and Jordan, 1800–1980* (Cambridge, 1987). On the commercialization of agriculture, see Owen, *Middle East in the World Economy*, 76–82, 153–72.

began to turn to the land as a more secure investment, and they wanted to own it outright.¹⁸ In response to these economic changes, the Ottoman state created the legal framework for the establishment of private property rights, rights that the state institutionalized in a new land code.¹⁹ Istanbul was conscious of the forces of change around the empire; the Turkish authorities realized they could not halt the gradual spread of private property, but they could regularize the empire's tax collection system on the land. Deeded property meant more efficiently taxed property, and increased tax revenues could help pay for Ottoman modernization schemes.

The gradual acquisition of private property rights by Syria's urban notables in the last decades of the Ottoman empire complemented already existing forms of landholding or land control with which these urban leaders had long been involved: the rights of usufruct (*tasarruf*) to state-owned lands (which were being regularized in this period), hereditary tax-farming, and control of pious trusts.²⁰ Notable families that became well-situated in the provincial government were able to manipulate the new legal system and accumulate significant landholdings, regardless of type. Although notables may not have completely consolidated their position as a class of absentee landowners before World War I, by the 1870s (or even earlier) there developed inextricable ties between large-scale landownership and political power, ties that could be observed as late as the 1950s in Syria and elsewhere in the Arab world.

The merger between the different urban status groups would be sealed by intermarriage. The wealthy, cosmopolitan families of the city center, who were so closely tied to the religious institutions, found it difficult to break with the past and move into the modernized bureaucracy established by the Ottoman state. By contrast, the military chiefs-cum-grain merchants in the peripheral quarters of the towns had less of a stake in the old order; and, powerless to resist a reinvigorated Ottoman state, they began to move deliberately into the administration. But, in towns such as Damascus and Aleppo, the great urban families of the city center possessed one thing that had always been desired by these social

¹⁸ The extent to which the general economy and, in particular, agriculture had become commercialized after mid-century varied from region to region. Reilly, for instance, suggests that "the economy of the Syrian interior was markedly less transformed than was that of the coastal regions, or was transformed considerably later"; "Status Groups and Propertyholding," 531. Also see Carol Frank, "The Transformation of Rural Society: The Syrian Interior, 1830-1930" (D. Phil. dissertation, Oxford University, 1989).

¹⁹ Recent scholarship on the impact of the Ottoman Land Code of 1858 suggests that the new registration system did not encourage the development of a new class of absentee landowners in certain regions of geographic Syria but merely "provided a new idiom for landholding and the registration of property"; Reilly, "Status Groups and Propertyholding," 529. Also see Haim Gerber, *The Social Origins of the Modern Middle East* (Boulder, Colo., 1987), chap. 5; and Peter Sluglett and Marion Farouk-Sluglett, "The Application of the 1858 Land Code in Greater Syria: Some Preliminary Observations," in Tarif Khalidi, ed., *Land Tenure and Social Transformation in the Middle East* (Beirut, 1984); Eugene Lawrence Rogan, "Incorporating the Periphery: The Ottoman Extension of Direct Rule over Southeastern Syria (Transjordan), 1867-1914" (Ph.D. dissertation, Harvard University, 1991), 301-07. Notables in Egypt acquired effective control of vast estates decades before the Ottoman Land Code of 1858, which merely codified already existing realities; see Kenneth Cuno, "The Origins of Landownership in Egypt: A Reappraisal," *International Journal of Middle East Studies*, 12 (1980): 245-75.

²⁰ See Roded, "Syrian Urban Notables," 380-81; Reilly, "Status Groups and Propertyholding," 528-31.

pariahs in the suburbs: a pedigree or high social status. Inter-marriage could bring benefits to both groups: social recognition to the *aghass* and easier access to government and to new economic opportunities that government provided to the religious families, who, incidentally, were already beginning to experience a loss of influence as their traditional monopoly of the legal and educational institutions decayed in the face of creeping administrative modernization and secularization.

Just after the mid-nineteenth century, religious leaders broke with social custom by supporting marriages outside their family networks. By the turn of the twentieth century, the major status groups that provided urban leadership in Syria had crystallized into a single elite with a similar economic base in land and political access through government office.²¹ A fairly stable local upper class emerged in the Syrian towns, as was true of the towns of Palestine and Lebanon—Jerusalem and Jaffa, Beirut and Tripoli.²²

The emerging class of landowning bureaucrats acquired other characteristics that helped to give it shape and continuity long after the collapse of the Ottoman empire. A distinctive Ottoman style of aristocratic behavior arose, which urban notables acquired as they were drawn more completely into the Ottoman system of rule. They traveled the new railroads to Istanbul for an Ottoman professional education in Turkish to prepare them to administer their provinces. They now spoke Turkish politely alongside Arabic. And they adopted the new upper-class dress of the Turkish rulers—the frock coat and the fez. These Ottoman trappings widened the social gap between the notables and the rest of local Arab society. The schools in Istanbul attended by the sons of the Syrian upper class were generally public administration schools, not military academies, which the sons of less well-established families attended.²³ Military careers were thought to be beneath the dignity and social position of the Syrian notables, and they used all kinds of connections to secure exemptions from Ottoman military service. The notables were to carry with them into the twentieth century this hostile attitude toward military service; ultimately, it contributed to their downfall after World War II.

The other characteristic that the notables carried with them into the twentieth century was an inclination to intense political factionalism. Their factionalism was not based on fictive or confessional alignments but on competition between rival, clan-based patronage alliances.²⁴ Linda Shatkowski Schilcher has vividly depicted the competition between the two major factions of notables that dominated urban

²¹ See Khoury, *Urban Notables and Arab Nationalism*, chap. 2; Roded, "Ottoman Service as a Vehicle for the Rise of New Upstarts."

²² See Yehoshua Porath, "The Political Awakening of the Palestinian Arabs and Their Leadership towards the End of the Ottoman Period," in Moshe Ma'oz, ed., *Studies on Palestine during the Ottoman Period* (Jerusalem, 1975), 351–81; Muslih, *Origins of Palestinian Nationalism*; Leila Tarazi Fawaz, *Merchants and Migrants in Nineteenth-Century Beirut* (Cambridge, Mass., 1983); Michael Johnson, *Class and Client in Beirut: The Sunni Muslim Community and the Lebanese State, 1840–1985* (London, 1986).

²³ See C. Ernest Dawn, "The Rise of Arabism in Syria," *Middle East Journal*, 16 (1962): 145–68; and Michael H. Van Dusen, "Intra- and Inter-Generational Conflict in the Syrian Army" (Ph.D. dissertation, Johns Hopkins University, 1971).

²⁴ This type of factionalism was prevalent both in Syria and in Palestine; see Salim Tamari, "Factionalism and Class Formation in Recent Palestinian History," in Roger Owen, ed., *Studies in the Economic and Social History of Palestine in the Nineteenth and Twentieth Centuries* (Carbondale, Ill., 1982), 177–202.

politics in Damascus in the first half of the nineteenth century, that is, before the merger. Based in the city center, with ties to the northern and western hinterland, one faction was pro-Ottoman and led by a family of secular dignitaries and former Ottoman provincial governors; the other was localist, incorporated greater numbers of the lower strata than did the city-center faction, and was led by the *aghass* of the Midan quarter, linked to the wheat-producing plains south of Damascus. The political economy of the period provides the key to understanding factionalism and the processes of political mobilization in Damascus. The first faction was supported by merchants engaged in the long-distance trade in luxury goods while the second faction was supported by merchants and artisans involved in local handicraft production and in the grain trade. The ability to adjust to changes and upheavals in the economy determined in large measure the relative strengths of each faction at different times.²⁵ Because the ulama, *ashraf*, secular dignitaries, and *aghass* were hierarchically stratified and thus unable to acquire their own independent political identity, they fell prey to the struggles between the two main competing factions in Damascus.²⁶

Long after the mid-nineteenth century, the intensity of urban political factionalism continued unabated. Members of the urban political elite evidently felt no obligation to close their ranks and clarify their common interests as a class on crucial political issues, because, until World War II, the Syrian urban leadership did not face serious local challenges from further down the social scale to the exclusive position that it had carved out for itself at the summit of politics in Syria.²⁷ It was not until the 1930s that the political elite began to sense a rising danger to its position from restless classes and forces further down in society. Only after Syria gained independence did this danger crystallize into organized movements to depose the veteran elite and break up the social bases of its power. The elite attempted to close ranks then, but it was too late.²⁸

To sum up, by World War I, the major political movements in Syria were led by members of urban notable families who brought to them a certain style of political action and a common way of looking at the world that they had acquired in late Ottoman times. The character of political life could be defined by the relative uniformity and sophistication of political culture in the towns. The characteristics of the local upper class were strikingly similar. In the towns, this class was composed of a socially integrated network of families whose material resource base was built on large-scale landholdings and who furnished the high-ranking bureaucrats and top local politicians as well as cultural and religious leaders.

²⁵ Schatkowski Schilcher, *Families in Politics*, 27–59, *passim*.

²⁶ Bodman has depicted the intensity of factionalism in Aleppo, Syria's other major urban center, in the early nineteenth century; *Political Factions in Aleppo*.

²⁷ The larger implications of the shift in the internal balance of power created by changes in the political economy and by the reinvigoration of Ottoman provincial authority require much more systematic examination, especially the other channels developed to articulate popular interests and demands once the notables began to redefine their traditional role as intermediaries between state and society as their interests became increasingly identified with the Ottoman state. See Khoury, "Urban Notables Revisited," 222, 225–26.

²⁸ In this sense, before the mid-twentieth century, the Syrian urban upper class was a "class in itself" but not a "class for itself." Hanna Batatu makes this very point in the case of Iraq in "Class Analysis and Iraqi Society," *Arab Studies Quarterly*, 1 (Summer 1979): 229–40.

Although the great urban families were of mixed ethnic stock—Arab, Turkish, and Kurdish—they were Arabic-speaking and belonged almost exclusively to the Sunni Muslim rite. The bonds of a common language and religion were strong enough to mitigate the potential for ethnic conflict within the class. Its qualities—its nature, style, and political behavior—pointed to a high level of social and cultural homogeneity. In a sense, its domination of urban society was legitimized because a large proportion of the population in the towns—a population that, despite the dramatic changes of the era, was still very much attached to its traditional religious beliefs, cultural practices, and customs—identified the local upper class as the defenders of the faith and guardians of culture as well as the providers of vital goods and services. In brief, a fairly cohesive Sunni Muslim upper class in the towns not only patronized but also represented a predominantly Sunni Muslim population, providing it with its cultural and religious leaders who embodied and articulated its beliefs and enforced its code of moral behavior.²⁹

ONE ADDITIONAL FACTOR CONTRIBUTED TO the Syrian urban leadership's ability to remain at the summit of local politics after the Ottoman empire collapsed: its seizure on the eve of World War I of the ascendant ideology of Arab nationalism. The emergence of Arab nationalism in the early twentieth century initially reflected broad changes in Arab society, especially within the urban upper class of Syria: the rising number of Syrians attending professional schools in Istanbul, increased exposure to European ideas, the accelerated pace of Ottomanization, and, after the ascent of the Young Turks in 1908, growing Turkish insensitivity to local Syrian needs. All these changes encouraged more frequent intercourse among the Arabs, greater interest in Arab history and culture, and the formation of Arab cultural clubs and secret political societies.

Urban notables were the group most essential to the development of Arab nationalism in Syria. The pioneering essays of C. Ernest Dawn have been most helpful in illustrating this point.³⁰ His explanation of the rise of Arab nationalism is in terms of a conflict within the Syrian urban elite. Arab nationalism arose as an opposition movement around the turn of the twentieth century and accelerated after the Young Turk revolution of 1908, when Turkish nationalists began to enforce administrative centralization, streamline the provincial bureaucracy, and replace Arabs with Turks in a number of important administrative posts in the Syrian provinces of the empire. A growing number of Syrian notables lost their jobs and thus their stake in the Ottoman system; it was they who first turned the dormant idea of Arabism into a vehicle for expressing their grievances with Istanbul and for regaining their positions. By contrast, many notables who managed to hold onto their posts supported the empire until the collapse of its authority in the Syrian provinces in 1918. Dawn's analysis has a certain Weberian ring to it: the bureaucratic "haves" versus the "have nots."

²⁹ Khoury, *Syria and the French Mandate*, 11–13.

³⁰ Many of Dawn's essays are collected in his 1973 volume, *From Ottomanism to Arabism*, although the most important ones first appeared in the 1960s.

Dawn's fundamental argument—that the emergence of Arab nationalism was a product of conflicts and factionalism within the landowning-bureaucratic class and that before 1918 it appealed only to a small but vocal minority of politically disenfranchised urban notables, mainly from Damascus—proved to be a major watershed in the study of the origins of Arab nationalism. He drastically revised the arguments of an earlier generation of writers who claimed that Syrian and Lebanese Christian intellectuals deserved the lion's share of the credit for the development of Arab nationalism and that its appeal was already widespread before World War I.³¹

Since the appearance of Dawn's revisionist essays and other works inspired by his line of argument,³² historians have brought new sources to bear on the study of the earliest phase of Arab nationalism in Syria that call for some revision of the revisionist thesis first elaborated by Dawn. Whereas Dawn examined the relationship between notables and the rise of Arabism from the vantage point of the Syrian provinces of the Ottoman empire, other historians have begun to treat this relationship from the vantage of Istanbul. Most scholarship, including Dawn's, has relied on a variety of sources that indicate what the Arabs have had to say about the Turks but tell us little about what the Turks had to say about the Arabs. Recently, M. Sükrü Hanioglu has uncovered Turkish documentary evidence that anti-Arab sentiments were present a decade before the Young Turk revolution of 1908 and were directly connected to an emerging Turkish nationalism that conflicted with the empire's reigning ideology of Ottomanism. These documents also indicate that Arabs were already being replaced by Turks in provincial administration as early as the 1890s. Arab notables may have begun to express their hostility to Turkish administrative practices in ethnic terms, of Arabs versus Turks, considerably earlier than Dawn has claimed.³³

Equally important, Rashid Khalidi has provided new evidence indicating that by World War I the Arab nationalist movement attracted a wider and more socially heterogeneous constituency than Dawn and other revisionists have recognized. The growth of the Ottoman state, with a modern army and administration, the spread of modern education through government schools, and the burgeoning of journalism offered new opportunities and created new demands for an expanding modern middle class, whose expectations grew more rapidly than its material interests. This middle class, or what Khalidi calls the "new intelligentsia" of journalists, teachers, professionals, and members of the military, found in Arab nationalism an attractive and useful set of ideas and was able, after 1908, to take part in an expanded political process, as a result of liberal Young

³¹ In particular, see George Antonius, author of the fascinating but flawed *Arab Awakening* (London, 1938). The most penetrating analysis of this book and its author is to be found in Albert Hourani's "The Arab Awakening Forty Years After," in Hourani, *The Emergence of the Modern Middle East* (Berkeley, Calif., 1981), 193–215.

³² For example, Cleveland, *Making of an Arab Nationalist*; Khoury, *Urban Notables and Arab Nationalism*, chap. 3.

³³ M. Sükrü Hanioglu, "The Young Turks and the Arabs before the Revolution of 1908," in Khalidi, *Origins of Arab Nationalism*, 31–49. Also see Hasan Kayali, "Arabs and Young Turks: Turkish-Arab Relations in the Second Constitutional Period of the Ottoman Empire (1908–1918)" (Ph.D. dissertation, Harvard University, 1988).

Turk measures.³⁴ This class also provided the Arabist movement with its most radicalized supporters.

Still another historian, David Commins, has suggested that Arabism's appeal before World War I also extended to a group of Muslim religious reformers in Damascus who shared with the Arab nationalist notables "a common political and social agenda." Moreover, while secular urban notables of the landowning-bureaucratic class were engaged in a growing struggle for power and influence with more privileged members of their own class who were identified with Istanbul and the status quo, religious reformers influenced by Islamic modernist trends emanating from Cairo were also engaged in an ideological struggle with high-ranking members of the Muslim religious establishment tied to Istanbul and who supported the status quo.³⁵ That some religious reformers supported Arab nationalist tendencies before World War I is yet another indication of the Arabist movement's appeal to more than an aggrieved group of secular notables with membership in the landowning-bureaucratic class.

By World War I, Arabism was rapidly becoming the ascendant political idea and movement of the times in Syria. Thus, during the war, when many notables began to jump from the sinking Ottoman ship, they grabbed, as they fell, the rope of Arab nationalism. They really had no other choice. It was this rope that enabled them to enter the interwar years with their political and social influence intact.

FOR THE MOST PART, MEN IMPORTANT IN LOCAL AFFAIRS in the last years of the Ottoman empire were the same men, or their sons, who wielded political influence under the French. Political leaders organized their personal support systems in interwar Syria as they had in late Ottoman times. Those with political power continued to be based in the towns and continued to extend that power to the settled countryside and eventually into the semi-nomadic areas.³⁶ The methods urban leaders used to acquire political power and their aims remained constant. Whatever the projected scope of power and whoever the political overlord, the basic building block of political influence in Syria remained the same: urban leadership.

The new system devised to replace Ottoman rule, and the brief post-World War I interregnum when the Hashemite Prince Faisal established an "independent" Syrian government in Damascus, was the mandate system.³⁷ Although this system, as conceived by the European powers after the war, was, in principle, to

³⁴ See Rashid Khalidi, "Social Factors in the Rise of the Arab Movement in Syria," in Said Amir Arjomand, ed., *From Nationalism to Revolutionary Islam* (Albany, N.Y., 1984), 53–70; and "Ottomanism and Arabism in Syria: A Reassessment," in Khalidi, *Origins of Arab Nationalism*, 50–69.

³⁵ See David Dean Commins, "Religious Reformers and Arabists in Damascus, 1885–1914," *International Journal of Middle East Studies*, 18 (November 1986): 420. This argument and related ones are elaborated in his *Islamic Reform: Politics and Social Change in Late Ottoman Syria* (New York, 1990).

³⁶ See Philip S. Khoury, "The Tribal Shaykh, French Tribal Policy, and the Nationalist Movement in Syria between Two World Wars," *Middle Eastern Studies*, 18 (April 1982): 180–93.

³⁷ On the character of politics and economy during the period of Faisal's Arab government, 1918–1920, see Khayriyya Qasimiyya, *al-Hukuma al-'arabiyya fi dimashq bayna 1918–1920* (Cairo, 1971); Malcolm B. Russell, *The First Modern Arab State: Syria under Faysal, 1918–1920* (Minneapolis, 1985); 'Ali Sultan, *Tarikh suriyya, 1918–1920: Hukm Faysal ibn al-Husayn* (Damascus, 1987).

be a benign form of political tutelage of so-called backward societies and not one of direct or indirect colonialism, in fact, the way the French administered Syria (and Lebanon) under mandate,³⁸ and the policies they pursued, had many important features in common with pre-war French colonialism.³⁹

Like any formal colony, Syria was expected to pay its own way financially. It had to foot the bill for the French army of occupation and the French administration. A typical system of colonial finance was imposed. Revenues came from indirect taxes, especially customs revenues, and a high proportion of government expenditure was on security and defense, not agriculture, industry, health, or education. Even though, according to the League of Nations, the overseer of all mandates, France was not to be granted special economic and commercial privileges in Syria, in practice this was disregarded. To complicate matters, the Syrian currency was tied to the French franc, which meant untold misery for the Syrian people in the 1920s and again in the late 1930s, when the franc fluctuated widely.⁴⁰ Syrians on a fixed income suffered most in these circumstances, in particular, bureaucrats and the wider intelligentsia—groups that could cause a foreign power all kinds of headaches when provoked.

The mandate system was not one of direct rule. The French, like the Turks before them, needed a partner in order to govern. For instance, the League of Nations obliged the mandatory power to prepare the Syrian people for independence; also, France's postwar economy was so fragile that it could no longer afford direct rule abroad. Nonetheless, the character of the new imperial authority differed significantly from the old: it was illegitimate and therefore unstable. France was not recognized to be a legitimate overlord, as the sultan-caliph of the Ottoman empire had been. The Turks had behind them four centuries of rule and the very important component of a common religious tradition.⁴¹ France had the dubious, even in Western eyes, legitimization conferred by the weak and imperfectly conceived mandate system. Because France had historically tried to establish and strengthen its position in the Levant by posing as the protector of the Christians and other religious minorities, the French were doubly distrusted by the Muslim majority in the region.⁴²

³⁸ General accounts of how France became installed in Syria and of French mandate policy can be found in Albert Hourani, *Syria and Lebanon: A Political Essay* (London, 1946); Pierre Rondot, "L'Expérience du Mandat français en Syrie et au Liban (1918–45)," *Revue de droit international publique*, 3–4 (1948): 387–409; Stephen H. Longrigg, *Syria and Lebanon under French Mandate* (London, 1958); Dhuqan Qarqut, *Tatawwur al-haraka al-wataniyya fi Suriyya, 1920–1939* (Beirut, 1975); Safiuddin Joarder, *Syria under the French Mandate: The Early Phase, 1920–27* (Dacca, 1977); André Raymond, "La Syrie, du Royaume Arabe à l'indépendance (1914–1946)," in Raymond, *La Syrie d'aujourd'hui*; Christopher M. Andrew and A. S. Kanya-Forstner, *The Climax of French Imperial Expansion 1914–1924* (Stanford, Calif., 1981); Khoury, *Syria and the French Mandate*. For a recent assessment of French policies in Lebanon during the early years of the mandate, see Meir Zamir, *The Formation of Modern Lebanon* (London, 1985).

³⁹ See Stephen H. Roberts, *The History of French Colonial Policy (1870–1925)* (London, 1929); Raymond F. Betts, *Assimilation and Association in French Colonial Theory, 1890–1914* (New York, 1961); Henri Brunschwig, *French Colonialism, 1871–1914: Myths and Realities* (New York, 1966).

⁴⁰ Khoury, *Syria and the French Mandate*, chaps. 2–3.

⁴¹ Hourani makes this point in "Revolution in the Arab Middle East."

⁴² See William I. Shorrock, *French Imperialism in the Middle East: The Failure of Policy in Syria and Lebanon, 1900–1914* (Madison, Wis., 1976).

Contributing to this instability of rule was the reality that the French did not have the resources to purchase loyalty on a large scale or to develop the Syrian economy either for the Syrians or for themselves. Syria was not resource-rich. Oil discoveries were insignificant, and cotton production faced a serious setback owing to America's world domination of cotton production and sales and to the development of synthetics in France and elsewhere. French monetary policies had disastrous financial repercussions. The devastation wrought by World War I, the debilitating reorientation forced on the Syrian economy by the partition of geographical Syria and the creation of distinct and separate mandatory regimes, and the continued erosion of Syrian industry by the spread of the European economy helped create and maintain a situation of high unemployment and inflation, which added to the political instability Syria faced in the interwar years.⁴³ Then there were specific French policies that inflamed traditional sectarian conflict by distinctly favoring the minorities and by promoting a series of administratively isolated minority enclaves in the face of local nationalist efforts to unify Syria.⁴⁴ The French threatened the Muslim majority by attempting to take over the organization of their institutions and debasing the symbols of their culture.

Above all, France ruled Syria with its eyes on its North African empire, not a focus that helped make a sound policy. Not only were many policies in Syria judged by their possible effects on North Africa, but the very categories by which Syria was understood were drawn from French experience there. These categories simply did not fit the Syrian situation. Syria under the mandate was not Morocco under the protectorate. The French could not create lasting stability by playing the minorities against the Sunni Muslim majority, the countryside against the towns, and local urban leaders against one another.⁴⁵

The French viewed Arab nationalism as a force that had to be resisted before it spread to North Africa and infected the heart of the French empire. In the early years of the mandate, they thought they could destroy Arab nationalism outright. Their approach differed from the British approach in the Middle East in the sense that the British knew early on that Arab nationalism was a force that would not wither away. Nor did the British think they could crush it outright. Rather, they tried to cultivate Arab nationalism, especially its more moderate leaders, by tying their interests to British interests and by having them contain, with discreet British encouragement, more radical Arab nationalists. Their method was particularly successful in Iraq during the interwar years, though less so in Palestine.⁴⁶

⁴³ See Sa'id B. Himadeh, *Monetary and Banking System of Syria* (Beirut, 1935); and Himadeh, ed., *Economic Organization of Syria* (Beirut, 1936); Youssef Khoury, *Prix et monnaie en Syrie* (Nancy, 1943); Badr al-Din al-Siba'i, *Adwa' 'ala al-rasmal al-ajnabi fi suriyya 1850–1958* (Damascus, 1958); 'Abdullah Hanna, *al-Haraka al-'ummaliyya fi suriyya wa lubnan 1900–1945* (Damascus, 1973); M. Abdul-Kader al-Nayal, "Industry and Dependency with Special Reference to Syria: 1920–1957" (Master's thesis, Institute of Social Studies, The Hague, 1974); Philip S. Khoury, "The Syrian Independence Movement and the Growth of Economic Nationalism in Damascus," *British Society for Middle Eastern Studies Bulletin*, 14 (1988): 25–36.

⁴⁴ See Itamar Rabinovich, "The Compact Minorities and the Syrian State, 1918–1945," *Journal of Contemporary History*, 14 (1979): 693–712.

⁴⁵ See Edmund Burke III, "A Comparative View of French Native Policy in Morocco and Syria, 1912–1925," *Middle Eastern Studies*, 9 (May 1973): 175–86.

⁴⁶ On British mandate policy in Iraq, see Peter Sluglett, *Britain in Iraq, 1914–1932* (London, 1976);

By the time the French understood the advantages of the British method, it was already too late.

The interwar years in Syria were pivotal in the sense that the country was uncomfortably suspended between four centuries of Ottoman rule and national independence. Nonetheless, politics continued to be organized much as it had been when Istanbul was Syria's overlord. Now, however, Syria's urban leadership was obliged to create a new balance of power between itself and the French. The notables—many of them newly grouped into an array of nationalist organizations and connected from town to town—still sought to rally the active forces of Syrian society behind their respective bloc or party; they still aimed to neutralize all local rivals; and they still wanted to appear as the sole figures of influence whose cooperation the French would have to gain in order to govern Syria.⁴⁷ The French, like the Ottomans before them, had to govern in association with groups from the urban upper classes. But, given the intrinsic illegitimacy of France's position and its penchant for dictatorial policy without regard for the position and interests of the local elite, many urban leaders became forces of opposition. They had to appear more as the spokesmen of the people in the halls of power than as agents of the French. To gain recognition from this strong-minded regime, they not only had to mobilize the popular forces in society but also had to seek broader political alliances than before between the different towns and regions, between the Sunni Muslim majority and the religious and ethnic minorities, and between themselves and like-minded Arab elites in the neighboring Arab territories of Lebanon, Palestine, and Iraq.⁴⁸ Hence, they required both a shared dedication of purpose and an ideology to express this new solidarity and drive. Nationalism provided the kind of ideological cohesion and emotional appeal that urban leaders needed to be politically effective between the wars.

Urban leaders shaped nationalism into an instrument by which to create a more desirable balance between themselves and the French. And they molded their movement to suit the particular interests of their class. In their hands, nationalism never was a revolutionary idea with profound social content; rather, it was a means to win French recognition without upsetting the status quo. Nationalism remained a simplistic idea: in its romanticized vision of the Arab past, it appealed

Hanna Batatu, *The Old Social Classes and the Revolutionary Movements of Iraq* (Princeton, N.J., 1978); Daniel Silverfarb, *Britain's Informal Empire in the Middle East: A Case Study of Iraq, 1929–1941* (New York, 1986). On British mandate policy in Palestine, see Bernard Wasserstein, *The British in Palestine: The Mandatory Government and the Arab-Jewish Conflict 1917–1929*, 2d edn. (Oxford, 1990); William Roger Louis, *The British Empire in the Middle East, 1945–1951: Arab Nationalism, the United States, and Postwar Imperialism* (Oxford, 1984), 383–572.

⁴⁷ These were the general aims of notables according to Hourani. See his "Ottoman Reform and the Politics of Notables."

⁴⁸ Recently, Daniel Pipes has made the case in *Greater Syria: The History of an Ambition* (New York, 1990) that Syrian nationalists were much more interested in unifying "Greater Syria," which comprises what is today Syria, Lebanon, Jordan, and Israel and the Occupied Territories, than in some wider pan-Arab unity. Indeed, some nationalists leaned in this direction, but, for the period of the French mandate, their influence within the overall Syrian independence movement was relatively small. Radical pan-Arabists posed the biggest challenge to the main nationalist leadership in interwar Syria, the National Bloc, and their interests ranged beyond "Greater Syria" to include Iraq, where these radicals had strong connections with like-minded Iraqi pan-Arabists. See Khoury, *Syria and the French Mandate*, chap. 15, *passim*.

to the hearts and minds of a broad section of Syrian society. It was an attractive and compelling ideology, more so because it was able to capitalize on Islam's political misfortunes by absorbing the important component of religious solidarity. The very language of this brand of nationalism indicated its class foundations. Apart from the aim of independence, interwar nationalism incorporated the liberal bourgeois language of constitutionalism, parliamentary forms, and personal freedoms.⁴⁹ Hardly a word was heard about economic and social justice—the last thing the nationalist landowners, still able to collect their generous rents from the ground, were interested in discussing.

Local politics was played out in the Syrian towns, where patronage systems cut through and deflected horizontal, class-based politics.⁵⁰ For much of the interwar era, nationalist leaders concentrated on mobilizing merchants, popular religious leaders, quarter bosses, and the urban crowd in their respective towns on an intermittent basis against the mandate authorities. However, the character of patronage also began to change in the 1930s, both in class and spatial terms. The base of the nationalist movement gradually shifted away from the popular classes in the old quarters, where religious chiefs and local gang leaders had traditionally mobilized the street; it shifted toward the new institutions such as the modern secondary schools, the university, the modern professional societies of lawyers, doctors, and engineers, the emerging scouting and sporting clubs and paramilitary organizations, and the infant trade unions, located in the newer, modern districts of the towns where many notables had also begun to relocate their residences in this period. Although this transition was by no means complete at the end of the mandate, by the late 1930s it was clearly irreversible.

For the political leadership, nationalism was certainly a useful instrument by which to mobilize forces in order to convince the French to bargain with them. But it also had the potential to alienate the French completely and destroy the delicate balance between French rule and the nationalists. Thus when the local leadership appealed to nationalism and behind it to religious solidarity, they had to temper their appeal by acts of "political prudence."⁵¹ In fact, nationalists preferred to mobilize urban forces on a temporary basis, that is, only when these forces could be useful to the elite's specific short-term aims. Mass mobilization on anything like a permanent basis was something to be avoided at all cost. At times, the elite feared mobilizing the streets, the mosques, and the schoolyards even more than they feared the French. Mass mobilization could get out of control and backfire in the face of the nationalist leadership. For decades, the absentee landowning class and its politically active members had worked within a somewhat fragile but comfortable framework as they played out their factional politics;

⁴⁹ Parliament was an ideal, genteel place in which factions could play out their struggles and ambitions. See Khoury, *Syria and the French Mandate*, chaps. 12–14, *passim*.

⁵⁰ See Philip S. Khoury, "Syrian Urban Politics in Transition: The Quarters of Damascus during the French Mandate," *International Journal of Middle East Studies*, 16 (November 1984): 507–40. On the role of youth organizations, including paramilitary groups, in the independence movement, see Khoury, *Syria and the French Mandate*, 406–14, 471–76. On the rise of the Syrian labor movement, see Hanna, *al-Haraka al-'ummaliyya fi Suriyya wa Lubnan 1900–1945*; and Elisabeth Longuenesse, "La Classe ouvrière en Syrie: Une Classe en formation" (3^{ème} cycle dissertation, Ecole des Hautes Etudes en Sciences Sociales, Paris, 1977).

⁵¹ Hourani, "Revolution in the Arab Middle East," 71.

under French rule, they rightly feared that politics might break out of that framework.

It did break out for nearly two years during the Great Revolt of 1925–1927, the most significant rebellion of the 1920s in the Arab world. This genuinely popular uprising among Druze peasants that spread to Damascus and beyond obliged nationalists to throw their weight behind the rebellion in order to hold onto their leadership. Although during the revolt they adopted the language of revolution and even some revolutionary tactics, they did so reluctantly, after more subtle and diplomatic efforts to bring about a relaxation of foreign control had failed. And, while their methods may have seemed revolutionary, their actual aim was to correct an unfavorable balance of power, not to break the yoke of French imperialism.⁵²

After two years, the revolt collapsed in the face of a reinforced French army of occupation, leaving the nationalist leadership in a much-weakened state. Although nationalists were relieved to drop armed confrontation as a strategy for gaining French recognition, their future hinged on French receptivity. Fortunately, the revolt's ferocity and duration had also convinced the French of the need to make some concession to the desire for self-government in Syria. This was an unforeseen opportunity for a defeated and demoralized nationalist leadership. They were able to adopt the more familiar tactics of periodic protest mixed with negotiation in an effort to restore the type of balance between foreign ruler and local leadership that had been operative in late Ottoman times.

The track to Syrian independence had now been cleared. Independence was not to be by popular struggle or revolt but by periodic protest coupled with diplomatic activity. In 1936, the nationalist leadership conducted a fifty-day general strike after several years of unsuccessfully trying to advance its aims of unity and independence. This was immediately followed by diplomacy in Paris that finally brought the nationalists into government for the first time and into a power-sharing relationship with the French. During World War II, the nationalists continued to conduct largely nonviolent strikes and demonstrations while they built a new set of delicate relationships, in this case with a third party, Britain, which during the war had come to hold the balance of power in Syria.⁵³ The

⁵² On the revolt of 1925–1927, see Elizabeth P. MacCallum, *The Nationalist Crusade in Syria* (New York, 1928); Adham al-Jundi, *Tarikh al-thawrat al-suriyya fi 'ahd al-intidab al-faransi* (Damascus, 1960); Munir al-Rayyis, *al-Kitab al-dhahabi li'l-thawrat al-wataniyya fi'l-mashriq al-'arabi: al-thawra al-suriyya al-kubra* (Beirut, 1969); McDowall, "Druze Revolt, 1925–27"; Joyce Laverty Miller, "The Syrian Revolt of 1925," *International Journal of Middle East Studies*, 8 (1977): 545–63; 'Abdullah Hanna, *al-Qadiyya al-zira'iyya wa'l-harakat al-fallahiyya fi suriyya wa lubnan (1920–1945)*, vol. 2 (Beirut, 1978); Edmond Rabbath, "L'Insurrection syrienne de 1925–1927," *Revue historique*, 542 (avril–juin 1982): 405–47; Khoury, "Reinterpretation of the Origins and Aims of the Great Syrian Revolt," 241–71; Khoury, *Syria and the French Mandate*, chaps. 7–9.

⁵³ See Hourani, "Revolution in the Arab Middle East," 71–72. On the nature of Anglo-French rivalry and the role of the Syrian nationalist movement during World War II, see Pierre Rondot, "Les Mouvements nationalistes au Levant durant la Deuxième Guerre Mondiale (1939–1945)," in *La Guerre méditerranée (1939–45)* (Paris, 1971); Geoffrey Warner, *Iraq and Syria, 1941* (London, 1974); Ahmed M. Gomaa, *The Foundation of the League of Arab States: Wartime Diplomacy and Inter-Arab Politics, 1941 to 1945* (London, 1977); François Kersaudy, *Churchill and De Gaulle* (1981; New York, 1983); Louis, *British Empire in the Middle East*, 147–72; Khoury, *Syria and the French Mandate*, chap. 23; A. B. Gaunson, *The Anglo-French Clash in Lebanon and Syria, 1940–45* (London, 1987); Avieli Roshwald, *Estranged Bedfellows: Britain and France in the Middle East during the Second World War* (New York, 1990).

nationalists preferred this way of achieving independence. Their methods did not upset the status quo, and they enabled veteran nationalists to take control of government after the French were obliged to withdraw from Syria in 1946.

ALBERT HOURANI HAS OBSERVED THAT, with independence, "the conditions for use of the old kind of political expertise" in Syria disappeared. For the first time, veteran nationalists no longer had to put themselves forward as brokers between the Ottoman state and local society or as popular leaders in the face of an alien authority like the French. They were now the rulers of their own country. Also, for the first time, they were in a position to use the state bureaucracy as "a means of coercion." In order to govern, the rulers of Syria saw no need to promote new local intermediaries with influence in society, as the Ottoman Turks and French had done before them. The age of a "politics of notables" gave way to a new age and a different type of politics.⁵⁴ But the old framework did not give way suddenly. The political leadership that had emerged in the nineteenth century from the urban absentee-landowning class did not immediately dissolve after generations of activity. It held on for nearly twenty years after independence.⁵⁵

Still, from its first days in the driver's seat in Damascus, the Syrian leadership's prospects were bleak. Already during the interwar period, rapid population growth, an inflated cost of living, the collapse of many traditional industries, the spread of modern education to the middle classes, and a changing intellectual climate had produced tensions and dislocations in urban society that eventually required more sophisticated responses than a narrowly focused nationalism provided. Increasing numbers of people had begun to seek support outside the old framework of patronage. Modern ideological parties headed by a rising generation posed a challenge to the old political order and to the veteran elite's monopoly of the nationalist idea. Its leaders belonged to emerging professional classes and organizations, were educated either in the West or locally, and found the Ottoman political legacy alien. Unlike the nationalist notables, who had resigned themselves to working within the political and administrative system established by the French, this ascendant generation adopted a more revolutionary strategy for achieving independence and unity. In the era of Syrian independence, members of these new groups contributed to the demise of the old way of politics. Indeed, it might be argued that they brought an end to politics altogether in Syria.

In any case, with independence, something had changed. Profound structural changes in the economy and society begun during the world depression and accelerated during World War II unleashed new forces with new methods and aims, which weakened the old framework. The politics of notables was replaced by a politics of "bureaucracies,"⁵⁶ but also by a politics of regionalism and, more

⁵⁴ Hourani, "Revolution in the Arab Middle East," 71.

⁵⁵ As Seale has persuasively argued in *Struggle for Syria*. Also see Michael Van Dusen, "Downfall of a Traditional Elite," in Frank Tachau, ed., *Political Elites and Political Development in the Middle East* (Cambridge, Mass., 1975), 115–55.

⁵⁶ Hourani, "Revolution in the Arab Middle East," 72.

precisely, by a politics of the countryside, of rural forces struggling against more established urban groups for control of the cities and of government.

As Hanna Batatu described it, “[R]ural people, driven by economic distress or lack of security, move into the main cities, settle in the outlying districts, enter before long into relations or forge common links with elements of the urban poor, who are themselves often earlier migrants from the countryside, and together they challenge the old established classes.” But, “in sharp contrast to the outcome of urban-rural conflicts of past centuries, the country people clinched a more enduring, if unstable, victory by virtue of their deep penetration of the Syrian army,” the army that the veteran elite had long snubbed.⁵⁷

The urban leadership lost power in Syria because it failed to wed nationalism to state power. Hourani has written that “new political ideas—radical nationalism, social reform, and Islamic assertion—provided the channels through which other social groups could pursue their interests: the growing middle class of the cities, teachers and students, and the army officers, many of them of rural origin and destined in the end to destroy the basis of the social power of the old elite, their control of the land.”⁵⁸ These new forces demanded the right to open up and take an active part in a political process that had previously been closed to them.

The veteran leadership eventually found it impossible to exclude the newly radicalized intelligentsia and members of the compact minorities—Druzes, Isma‘ilis, and especially ‘Alawis—who came from peasant and lower-middle class origins and from the rural periphery and smaller towns. Nothing could prevent them from redefining their relations with one another and with the veteran elite in government. On the political level, these new forces gravitated toward modern political organizations—the Communists, the Muslim Brotherhood, the Syrian Social Nationalist party, and the Baath party, which had begun to make their ascent in the years before independence.⁵⁹

With their more rigorous systems of ideas and sophisticated methods of organization, they criticized and challenged the veteran elite in several concrete ways: for failing to uphold the reigning idea of pan-Arab unity, for contributing to the Arab failure to save Palestine in 1948, and for retaining strong, compro-

⁵⁷ Hanna Batatu, “Some Observations on the Social Roots of Syria’s Ruling Military Group and the Causes for Its Dominance,” *Middle East Journal*, 35 (Summer 1981): 337–38.

⁵⁸ From Hourani’s foreword to Khoury, *Syria and the French Mandate*, xii. For a detailed analysis of the way these new forces made their ascent onto the Syrian political stage, see Hinnebusch, *Authoritarian Power and State Formation in Ba’thist Syria*, chap. 4, *passim*.

⁵⁹ On the origins of the Syrian Communist party, see Batatu, *Old Social Classes and the Revolutionary Movements of Iraq*, chap. 24. On the origins of the Syrian Muslim Brotherhood, see Johannes Reissner, *Ideologie und Politik der Muslimbrüder Syriens* (Friburg, 1980); Hanna Batatu, “Syria’s Muslim Brethren,” *MERIP Reports* (Middle East Research Information Project), no. 110, 12 (November–December 1982): 12–20, 24; Olivier Carré, *Les Frères musulmans: Egypte et Syrie, 1928–1982* (Paris, 1983); Umar F. Abd-Allah, *The Islamic Struggle in Syria* (Berkeley, Calif., 1983), chap. 3. On the origins of the Syrian Social Nationalist party, see Seale, *Struggle for Syria*, chap. 8; Labib Zuwiyya Yamak, *The Syrian Social Nationalist Party: An Ideological Analysis* (Cambridge, Mass., 1966). On the origins of the Baath party, see Kamel S. Abu Jaber, *The Arab Ba’th Socialist Party: History, Ideology, and Organization* (Syracuse, N.Y., 1966); Nabil M. Kaylani, “The Rise of the Syrian Ba’th, 1940–1958,” *International Journal of Middle East Studies*, 3 (January 1972): 3–23; John F. Devlin, *The Ba’th Party: A History from Its Origins to 1966* (Stanford, Calif., 1976); Batatu, *Old Social Classes*, chap. 38; Olivier Carré, “Le Mouvement ideologique ba’thiste,” in Raymond, *La Syrie d’aujourd’hui*, 185–224.

mising ties to the Western powers.⁶⁰ These radicalized forces shifted the emphasis of nationalism to better accommodate the accelerated changes taking place in economy and society. The language of nationalism no longer emphasized constitutionalism, parliamentary forms, and personal freedoms but rather social and economic justice for the masses, neutralism in international politics, and, in the case of the eventually triumphant Baath party, pan-Arab unity. Nationalism stressed mass education, land reform, social welfare, and rapid industrialization, all of which were to be brought about by a strong, dynamic interventionist state, for the good of society. Government was to be “for the people, but not by the people.”⁶¹

Hourani has written that “in this new age, the political struggle takes place on two levels. Those inside the system of government compete for favoured access to the ruler and control of important positions of power in administration. Those outside must aim at a total overthrow of the government, using the only method which seems likely to be effective: the armed forces.”⁶² Control of the armed forces went to the rural people—members of the compact minorities—in particular, the ‘Alawis, a dispossessed mountain and hill community armed with a strange, heterodox brand of Islam, who were fiercely tribal and, as Raymond Hinnebusch notes, “the most intense carriers of peasant grievances against the urban establishment.”⁶³ The French had encouraged them to join the army during the mandate; they saw the military as the one avenue for social advancement beyond the rural squalor and isolation in which they lived.⁶⁴

Once the ‘Alawis began to penetrate the military in significant numbers, they used their rural, regional, tribal, and religious solidarity to monopolize its levers of command. At first, they aligned themselves with rural Sunni Muslims in the armed forces to weaken Sunnis from the towns, who were in control of the army immediately after independence. Then the ‘Alawis turned on their rural Sunni allies and the smaller minority communities such as the Druzes in the army.⁶⁵ Finally, the ‘Alawis fought out their own internal struggles, with the cleverest faction in the military winning out, the one headed by the current president of Syria, Hafiz al-Asad.⁶⁶ That members of the veteran elite managed to retain influence in politics until the early 1960s suggests just how tenacious and resilient they were in the face of the growing radicalized movements in the military and in

⁶⁰ See Seale, *Struggle for Syria*. This same pattern could be detected in the cases of Egypt and Iraq in the late 1940s and 1950s.

⁶¹ See Albert Hourani, “Middle Eastern Nationalism Yesterday and Today,” in Hourani, *Emergence of the Modern Middle East*, 179–92; Philip S. Khoury, “Islamic Revival and the Crisis of the Secular State in the Arab World,” in Ibrahim Ibrahim, ed., *Arab Resources: The Transformation of a Society* (London, 1983), 213–36.

⁶² Hourani, “Revolution in the Arab Middle East,” 72.

⁶³ Hinnebusch, *Authoritarian Power and State Formation in Ba’thist Syria*, 160.

⁶⁴ On the relationship of the compact minorities to the Syrian army, see Van Dusen, “Intra- and Inter-Generational Conflict in the Syrian Army”; Itamar Rabinovich, *Syria under the Ba’th, 1963–66: The Army Party Symbiosis* (Jerusalem, 1972); Nikolaos van Dam, *The Struggle for Power in Syria: Sectarianism, Regionalism, and Tribalism in Politics, 1961–1978* (London, 1979). On the ‘Alawi religious sect, see Jacques Weulersse, *Le Pays des Alaouites* (Tours, 1940); Fuad I. Khuri, *Imams and Emirs: State, Religion and Sects in Islam* (London, 1990), chaps. 9, 15.

⁶⁵ See Batatu, “Some Observations on the Social Roots of Syria’s Ruling Military Group,” 331–44.

⁶⁶ On Asad’s rise to power and subsequent career, see Patrick Seale, *Asad of Syria: The Struggle for the Middle East* (Berkeley, Calif., 1989).

civilian society. But, in the end, they were swept aside. With their departure, an old way of political life disappeared in Syria.

Today, there are fewer and fewer reminders of the time when Syrian political culture and the urban notables were closely identified with one another. For the historian, perhaps the most vivid is the 'Azm Palace in the Suq Saruja quarter of Damascus, which had been the ancestral residence of an important branch of the leading notable family of the town for nearly two centuries. It is now home to Syria's national historical archives. Here, the historian can examine a partial but illuminating record of the achievements and failings of Syria's urban notables in some of the very rooms in which they planned their activities. Could there be a more appropriate place in which to reconstruct Syria's political past?

The Baath Party: Rise and Metamorphosis

JOHN F. DEVLIN

THE ARAB SOCIALIST BAATH (RESURRECTION) PARTY began its half-century of existence as a movement standing for Arab nationalism, freedom from foreign rule, and the establishment of a single Arab state. For the past quarter-century, it has been represented by two mutually hostile authoritarian regimes, those in Iraq and Syria. Each claims to be the sole legitimate inheritor of the original Baath legacy. Their antagonism is so deep that Syrian forces joined the coalition to liberate Kuwait, going into action against the Iraqis in the short combat that ended the Iraqi occupation. This essay examines these two phases of the Baath party's existence and explores why and how the second phase developed from the first. It concludes with a brief assessment of the two single-party states at this time of domestic and interstate political ferment in the Arab world.

Pan-Arab nationalism was the foremost ideology of the Arab world for half the twentieth century, from the end of World War I to the late 1960s. Its origins reach back into the nineteenth century, as Albert Hourani has lucidly shown us.¹ As a political force, Arab nationalism may be said to have begun in the decade immediately prior to World War I, when Arab subjects of the Ottoman empire began to assert their Arabness in opposition to the Turkish focus of the Committee of Union and Progress in its governance of the Ottoman state after 1908. Of success these Arabs had little until the Allies had defeated the Ottomans and set up several Arab states in the former eastern Arab *wilayets* (provinces) of the defunct empire. This change opened the way for Arabism to function as a legitimate political ideology in those entities. "Arab nationalism became the creed of all political activists everywhere in the Fertile Crescent except Lebanon." However, the same class of notables that had been locally dominant under the Ottomans continued to wield extensive power.² As a result, Arab nationalism came to be defined by those in political opposition, and, with such definition, it progressively became their property. *They* were the nationalists; the notables of the ruling elite were agents of imperialism.

The energies of the political elite during this interwar period were necessarily focused on gaining independence from their mandate overlords. In time, their interest and attention shifted away from general Arab affairs to the state in which

¹ Albert Hourani, *Arabic Thought in the Liberal Age, 1798–1939* (London, 1962), chap. 11.

² C. Ernest Dawn, "The Formation of Pan-Arab Ideology in the Inter-war Years," *International Journal of Middle East Studies*, 20 (February 1988): 80.

the struggle for independence and their own political fortunes were situated.³ By the end of World War II, the system of Arab states within boundaries drawn in European chanceries in the early 1920s was well on its way to general acceptance by dominant political forces. The Arab League, founded in 1945, consisted of seven sovereign states. To be sure, expansionist ambitions existed; the Hashemites in Iraq and Transjordan cast covetous eyes on Syria, and the growth of the Saudi state was stopped at the borders of British-protected principalities. Arab nationalists in the Fertile Crescent saw the ruling elite as compromising full Arab independence by its willingness to cooperate with Britain or France. Getting rid of entrenched political elites was seen by the founders of the Baath movement as an entirely logical and necessary step on the road to achieving pan-Arab nationalism.

During the same interwar period, the concept of who was an Arab and what were the nation's boundaries underwent a major change. As of World War I, Arabs thought of their nation as existing solely in Asia. Husain ibn 'Ali, sharif of Mecca, in his 1915 correspondence with the British high commissioner in Cairo, defined the western boundaries of "the Arab countries as the Red Sea, the Mediterranean Sea up to Mersina."⁴ In the 1920s and 1930s, Arabism came to be defined by language rather than by geography; an Arab was a person whose native tongue was Arabic. People had created the Arab nation, which had spread out, in the view of nationalist writers, from its heartland in much earlier times. Where Arabs lived and spoke their language, whether east of the Mediterranean or in North Africa, there was the Arab nation. The limitations imposed by the geographic concept from World War I of an Arab state or states east of the Mediterranean and Red Sea had disappeared twenty-five years later. When the Arab League was founded in 1945, one of its seven members (as it happened, the largest and strongest), Egypt, was in Africa. The league reached the Atlantic with the admission of Morocco in 1958.

The founders of the Baath party were products of this interwar growth of nationalist sentiment. Michel 'Aflaq, Orthodox Christian, and Salah al-Din Bitar, Sunni Muslim, born in 1910 and 1912 respectively to Damascene merchant families of middling status, attended secondary school in the 1920s and university in the 1930s.⁵ They accepted wholeheartedly the Arab nationalist ideology to which they had been exposed in school, pan-Arabists having been especially influential in the school systems of Syria and Iraq in those years. In 1942, the two friends abandoned their own teaching careers to devote themselves full-time to the creation of a movement dedicated to achieving freedom (*hurriyah*) from foreign control and the unity (*wihdah*) of all Arabs in a single state. To these goals, the Baathists added socialism (*ishtirakiyah*), which they interpreted as social justice for the poor and underprivileged. The slogans "Unity, Freedom, Socialism" and "One Arab Nation with an Immortal Mission" appeared on party publications

³ Philip S. Khoury, *Syria and the French Mandate: The Politics of Arab Nationalism, 1920–1945* (Princeton, N.J., 1987), chap. 21.

⁴ Cited in Sylvia G. Haim, *Arab Nationalism: An Anthology* (Berkeley, Calif., 1964), 90.

⁵ John F. Devlin, "Michel Aflaq" and "Salah al-Din Bitar," in Bernard Reich, ed., *Political Leaders of the Contemporary Middle East and North Africa: A Biographical Dictionary* (New York, 1990), 32–39; 108–12.

from the mid-1940s and continue to appear on the mastheads of the Baath newspapers in Syria and Iraq.

'Aflaq and Bitar initially thought of themselves as leading a movement capable of inspiring Arabs with the ideas of Arab nationalism.⁶ 'Aflaq in particular believed that the Baath's primary function was to educate people through pamphlets, lectures, and meetings. Others among the handful of early associates, including Bitar, believed that a political organization was necessary for the ideas of the group to be truly influential. In time, 'Aflaq agreed, and by mid-1945, the Baathists made their first application to form a political party. The government rejected it. The movement continued to call itself a party, however, and gained permission to function as one and to publish a newspaper (*Al-Ba'th*) in mid-1946, shortly after French troops had left Syria. The formal founding congress of the Baath party took place on April 4–6, 1947, in Damascus, with about 200 attendees. A number were Arabs from countries other than Syria, for membership was open to all from the "Arab fatherland [which] constitutes an indivisible political and economic unity" according to the first Fundamental Principle of the party constitution adopted at this congress.⁷

The congress brought into the party a group not directly tied to 'Aflaq and Bitar, many from the Latakia governorate, among them numerous 'Alawis. These men (and boys, for many were students, as were many of 'Aflaq's immediate followers) shared the Baath party's goals of Arab independence and Arab unity. They differed in two respects. First, they looked to Zaki Arsuzi, an 'Alawi expelled from the Sanjak of Alexandretta for opposing French policy, which ended in any case with the transfer of the Sanjak to Turkey in 1939. As a teacher, he spread the ideas of Baathism via the classroom and attracted a circle of followers. A proud man, Arsuzi was influential intellectually but not organizationally; he and 'Aflaq appear to have disliked each other.⁸ His disciples were led to combine with those of 'Aflaq and Bitar by another 'Alawi exile from Alexandretta, Dr. Wahib al-Ghanim. Ghanim added recruits of his own. One was a sixteen-year-old from the hill village of Qardaha, who had come to the provincial capital to attend school. His name was Hafiz al-Asad.⁹

The second difference between the groups was over the issue of socialism. Ghanim was deeply concerned about the wretched conditions of many rural Syrians, which he came to know as an itinerant doctor in the villages of the Latakia district. He blamed the ruling elite for these conditions and insisted on the inclusion in the 1947 party constitution of clauses calling for social justice: the limitation of agricultural holdings, worker participation in management, and state ownership of heavy industry, natural resources, and public utilities. While 'Aflaq concentrated on nationalism and the issue of unity, he was also aware that the economic as well as the political power of the notables in Syria, and by extension in other states, had to be broken if the Baath's vision of independence was to be

⁶ See John F. Devlin, *The Ba'th Party: A History from Its Origins to 1966* (Stanford, Calif., 1976).

⁷ Text of the constitution is in Devlin, *Ba'th Party*, 345–52; and Haim, *Arab Nationalism*, 233–41.

⁸ Years later, the 'Alawis from Latakia, who had come to dominate the regime in Damascus, remembered Arsuzi, who died in 1969, and named a school for him.

⁹ Patrick Seale, *Asad of Syria: The Struggle for the Middle East* (London, 1988), 34–35. The author's long experience in Syria and direct access to Asad make this an especially valuable study.

achieved. While he had resisted Ghanim's insistence on including socialist or social justice goals in the Baath constitution, by early in the 1950s he and Bitar agreed to amalgamate the Baath party with the Arab Socialist party of Akram Hawrani, which shared some of Ghanim's concerns. Although he came from a landowning family of Hamah, Hawrani turned against his class and organized the landless or impoverished peasants of the district as his political base. In the mid-year elections of 1954—the freest in independent Syria's history—Hawrani's followers accounted for one-third of the dozen and a half seats that Baathists won.

As the first Arab political party with pan-Arabist goals, the Baath had a leading role in spreading the doctrine of unity in the 1950s. In the party newspaper they edited, 'Aflaq and Bitar hammered away at the theme of unity and the "sacred mission" of revitalizing Arabdom. Party branches were founded in Lebanon, Iraq, and Jordan in 1948–1951, and adherents were recruited even farther afield. A new force appeared in the Arab east with the seizure of power by the pro-nationalist Free Officers Society in Egypt in 1952. Nasser emerged as the leader among them in 1954, took his place among the nonaligned leaders at Bandung in 1955, and made an arms deal with the USSR in the same year. At about the same time, he adopted Arab nationalism as a key element of Egypt's external policy. Once he had done so, the combination of his personality, his appeal to the masses, and Egypt's position as the wealthiest and most populous Arab state and a focus of Arab culture sent the popularity of Arab nationalism skyrocketing among hitherto-unmobilized opposition groups throughout the Arab world. The Baath leaders, who had harbored strong doubts about Nasser's Arabism during his early years in power, overcame their hesitancy and announced on April 17, 1956, that the party would work for the union of Egypt and Syria as a first step toward Arab unity.

SYRIA HELD A PARTICULAR INTEREST for Egypt, an interest that went back centuries. Muhammad 'Ali had taken control of the area from the Ottomans in 1832 and held it for a decade until forced out by European pressure. Iraq, too—or whatever power had controlled the Baghdad and Mosul areas—traditionally sought a friendly or subservient state in the area of the trade routes from the Tigris-Euphrates Valley to the Mediterranean. The interests of the two riverain powers often clashed in regard to Syria. They did so after World War II, when Egypt bested Iraq to dominate the new Arab League. The contest continued for a dozen years; Syria in those years was what Lebanon became in the 1970s and 1980s, namely, an arena in which other area powers jostled for supremacy.¹⁰

The meddling by Iraq and Egypt, plus an increasing factionalization of the Syrian officer corps, the ultimate domestic arbiter of Syrian political life from 1949 to 1958, brought the issue of Arab nationalism and unity to a head in early 1958. The officers, badly divided by the coups and counter-coups of the previous decade, proved unable to agree on how to share power among themselves. A

¹⁰ Patrick Seale, *The Struggle for Syria: A Study of Post-War Arab Politics, 1945–1958* (New Haven, Conn., 1986), is the classic treatment of this subject.

majority of politically active officers, strongly Arab nationalist in sentiment, and from the postwar military academy classes of 1947–1949, turned to Nasser, by then the foremost Arab nationalist, for a solution. Vigorously supported by the Baath leaders, a delegation of officers went to Cairo in January and asked Nasser to agree to the union of Syria with Egypt. The Egyptian leader hesitated; influence over Syria was important to him, but union carried with it potential problems. It was a tempting offer, however, a testimony to his rapid rise in importance. Moreover, failure to honor a request for unity would call into question his own Arab nationalist credentials. After thinking it over, he agreed on condition that all political parties in Syria dissolve in favor of a single, mass party modeled on the Egyptian National Union of 1956. Syria's party leaders agreed to this stipulation, some with considerable misgivings, except for the Communists, who chose to go underground. Egypt and Syria became the United Arab Republic (UAR) on February 1.

The Baathist leaders accepted Nasser's condition enthusiastically. 'Aflaq stated, "We will be officially dissolved but we will be present in the new unified party, the National Union. Born of the union of two countries, this movement cannot be inspired by principles other than those of the Ba'ath."¹¹ This was a colossal miscalculation. Nasser was a master at political maneuvering; 'Aflaq and Bitar were not. Influential as it was in forming opinion and promoting Arab nationalism, the Baath party in Syria had only four or five thousand members, concentrated in certain towns and districts. When the National Union elections were finally held in July 1959, the Baath organization had been defunct for sixteen months; "former Baathists" won less than 200 of 9,445 seats on local councils. Lasting damage was done to the pan-Arab nature of the party and to its leaders' reputations by the manner of the party's dissolution. 'Aflaq, Bitar, and Hawrani agreed to dissolve the party without consulting others on the Baath Party National (Pan-Arab) Command or on the Syrian Regional Command as required by party statutes.¹² In Syria, party members were stunned at the move, the party organization collapsed, and the three leaders received an irreparable blow to their prestige and influence.

The UAR episode constituted a major turning point in the history of the Baath party. First of all, this experiment in combining two Arab states in one failed. Syrians quickly came to resent the way in which Nasser's appointees dominated all aspects of policy and administration. The union cabinet sat in Cairo, and decisions were made there. In September 1959, Nasser sent a close Egyptian colleague to govern Syria with the help of a small coterie of pro-Nasser Syrian military officers. The previous ruling elite was excluded from power and its economic base hurt by a land reform program begun in the same year. On September 28, 1961, major units of the Syrian army rose in rebellion, declaring that Syria was seceding from

¹¹ *L'Orient* (Beirut), February 25, 1958.

¹² Growth led the Baathists to adopt a formal organization in 1954. The pyramidal structure rested on units in city quarters or rural districts; above that came a branch for each province with its elected command, then a regional command and a national command. The terminology reflects the Baath view that the entire Arab world was one nation divided into artificial political units, which should be considered regions of the whole.

the UAR. Nasser's instinct was to resist, but with few Egyptian troops there and little chance of sending sufficient forces quickly, he accepted the inevitable.

Second, Syria's secession was a powerful demonstration that the states created after World War I had developed great staying power. That staying power had already been shown in Iraq three years earlier, when enthusiasm for Arab unity ran high among opposition groups in eastern Arab states. On July 14, 1958, a group of Free Officers overthrew the monarchy in Iraq. Several of its leaders rushed to Damascus, where they declared their interest in joining the UAR. However, the senior officer of the group, 'Abd al-Karim Qasim, who remained in Baghdad, moved efficiently to isolate the pan-Arabist officers and to bolster his personal position.¹³ He succeeded, and Iraq kept its distance from the Arab community during his five years in power. Now Syria, which had for many years enjoyed the distinction of harboring the strongest advocates of pan-Arabism, had forcibly rejected unity. There remained many Syrians and other Arabs who held fast to the goal of unity; some were Baathists and others Nasserists. But the appeal of Arab unity, which had been uncritically espoused by opposition groups in the eastern Arab states as a solution to Arab problems, received a blow from which it has not recovered.¹⁴

Third, major changes occurred in the leadership and membership of the Baath party. Many members in Syria, shocked at the abrupt dissolution of the party and deprived of an organizational connection, drifted away and did not return in significant numbers when the party was reconstituted in 1962–1963. Hawrani went his own way. No member of the pre-UAR Syrian Regional Command served on that body after secession. In addition to 'Aflaq, only one pre-UAR member (a Lebanese) remained on the National Command by 1960. In both Iraq and Jordan, the secretaries of the Regional Commands left the Baath to form pro-Nasser organizations. These wholesale changes opened the way for a new generation to emerge in the party's National Command and in the four principal Regional Commands—Syria, Iraq, Jordan, and Lebanon—as well. This generation was on average about twenty years younger than 'Aflaq and Bitar and their contemporaries. Its formative years were the 1940s rather than the 1920s, and its members tended to have greater concern for the socialist component of Baath ideology than did the party's founders.

Finally, failure of the Baath leaders to consult elected party bodies before ordering the dissolution set a precedent that helped move the Baath party from an organization with elected commands to one rigidly controlled from the top. The original structure had a pyramid of commands organized by district, branch, region, and nation and chosen by delegates elected to the relevant congresses. 'Aflaq, Bitar, and Hawrani thought of the Baath as their party and were often authoritarian in running its affairs, but they did have men with differing views serving on the various commands. After the UAR, military men emerged on the Regional Commands in Iraq and Syria; their training in a chain-of-command

¹³ Phebe Marr, *The Modern History of Iraq* (Boulder, Colo., 1985), 159–60. A good overview of Iraq since World War I.

¹⁴ Muammar Gaddafi of Libya, who was a schoolboy during the Arab nationalist heyday of the 1950s, was unique during the 1970s and 1980s as an Arab head of state who was a true believer in pan-Arabism.

system disposed them to authoritarian control from the top. It was not a long step from having Baathist officers as members of a regional command to having officers dominating a command through control of military power. Nor was the next step, centralization of power in one man holding the offices of Baath party head, military commander-in-chief, and head of government. Before that happened, selection had replaced election as the Baath method of filling party commands.

The party's haphazard recruiting practices resulted in Baathists turning up in field grade (major, colonel) officer levels in the Syrian and Iraqi armies by the end of the 1950s. Young men typically were indoctrinated in political ideology by secondary school teachers in the expanding public school system. Poorly paid, such teachers were rarely apologists for the ruling elite. Hafiz al-Asad joined the Baath in secondary school, as did his close colleague, Syrian defense minister Mustafa Talas, a Sunni from a village near Homs. So did most of their Baathist contemporaries. Asad and Talas found 15 fellow Baathists or sympathizers out of 110 students in their class when they entered the Homs Military Academy in 1951.¹⁵ The process of recruitment was similar in Iraq, although more rigorous control by the regime hindered political activity and organization. Also, the Baath organization lagged behind that in Syria by some eight years.

Syrian Baathists in the military built an organization of the deepest significance for Syria's future during the short life of the UAR. In extending his authoritarian governing system to Syria, Nasser was extremely wary of the problems that political activity on the part of military men could cause. He was familiar with the history of military involvement in Syrian politics—three coups in 1949, three more in the 1950s, including the move that delivered Syria to Egypt. Moreover, he was a successful coup plotter himself. Under the rubric of integrating the Egyptian and Syrian armed forces, hundreds of officers who had records of political activity were posted to Egypt. Among them were a score or two of Baathists, mostly captains and majors. In late 1959, five formed a Baath Military Committee; Asad and Salah Jadid were to be the most prominent. In the next two years, it expanded to a dozen members, with twice that many sympathizers in Egypt and Syria. The committee leaders were in Egypt when the secession occurred. Even though they had no part in it, they were well placed to participate in the military-political maneuvering that followed, far better placed than the civilian Baath leaders who only began active reorganization in mid-1962.

The Military Committee joined forces with a faction of non-Baath officers and seized power on March 8, 1963. Over the next several months, committee members purged and exiled their non-Baath associates and brought civilian Baathists into the government. On the surface, it appeared that the pre-UAR Baath party was recovering its lost status. 'Aflaq and his close Syrian followers enjoyed strong support in the National Command. As the senior Baathist politician with the most experience, Bitar became prime minister. Within Syria, however, they had little support. A majority of returning or newly recruited party members blamed the old leaders for the UAR fiasco; these members wanted social

¹⁵ Lucien Bitterlin, *Hafez el-Assad: Le Parcours d'un combattant* (Paris, 1986), 34. Seale, *Asad*, 39, gives the class size as ninety.

and economic change in Syria rather than more talk of pan-Arab unity. The Syrian Regional Command quickly came under the control of a mixed military-civilian group of these "regionalists," so-called because they considered the Syrian region and not the Arab nation the appropriate focus for the Baath's efforts. 'Aflaq, viewing Syrian matters from his position as party founder and secretary-general, had support in the Iraqi and other regional organizations. Content to have the party come to power through military force, he expected the regionalists and the military Baathists especially to take orders from the National Command. Most of them did not agree, and the stage was thus set for a power struggle.

The contest took three years to resolve. It was fought out in three National (pan-Arab) Congresses and five separate sessions of two elected Syrian Regional Congresses. Although some of his colleagues from other states had their doubts about 'Aflaq's tactics, he retained their support. Within Syria, the regionalists were supreme and remained firmly in control of the Regional Command. 'Aflaq himself gained some allies in the military, which led him to overestimate his strength and to dismiss the Regional Command at the end of 1965. The Military Committee Baathists made their preparations and on February 23, 1966, ousted the 'Aflaq-Bitar faction by force. In a short time, all were in exile.¹⁶ Although the issues that precipitated this development were exclusively ones of policy and power in Syria, the coup of February 23 split the Baath party in two. Most Iraqi Baathists continued to recognize 'Aflaq and his colleagues as the legitimate leadership. Once they had won power in Baghdad, the Iraqi Baathists offered asylum and the assurance of continued titular leadership to 'Aflaq and his Syrian associates.

In Syria, the winning coalition of officers and their civilian Baathist allies soon fell out over issues of policy and power. By the end of 1966, most of the half-dozen second-echelon members had been driven out of the army; one even sided with the 'Aflaq faction out of frustration at having failed to receive the senior military post to which he aspired. Intra-Baath politics in Syria settled into a contest between two factions. That led by Salah Jadid espoused a doctrinaire socialist system domestically and support externally for a Palestinian war of national liberation against Israel. Jadid had resigned his post as army chief of staff in 1965 and directed the Baath regional party bureaucracy with the aid of many like-minded civilians. His challenger was Hafiz al-Asad, who was more concerned with results than with doctrine in domestic affairs. Defense minister Asad was shaken by the smashing defeat of Syrian forces by Israel in June 1967 and had nearly been sacked for it by his colleagues. If he were to bear responsibility for such matters, he believed that he should have commensurate authority.¹⁷ Through control of military postings and promotions, he patiently moved into an impregnable position. On being informed in November 1970 that a party congress had voted to remove him from his party and government posts, Asad

¹⁶ Itamar Rabinovich, *Syria under the Ba'th 1963-66: The Army Party Symbiosis* (Jerusalem, 1972), covers this period in detail. Nikolaos van Dam, *The Struggle for Power in Syria: Sectarianism, Regionalism and Tribalism in Politics, 1961-1980* (1979; London, 1981), is also a valuable study.

¹⁷ Seale, *Asad*, 142-45.

took over the government in a bloodless coup in order to “correct,” as he put it, his opponents’ deviations.

THE SUCCESS OF THE REGIONALISTS in 1966 and Asad’s takeover in November 1970 placed Syria squarely in the hands of men of rural origin. During the 1940s and 1950s, the Baath message of pan-Arabism appealed to many levels of Syrian society, but its socialist or social justice message was most attractive to those who believed that they were not receiving a fair share of the state’s resources. In Syria, a state that lacked an industrial proletariat of any size, that meant rural people. As it happened, three of the five founders of the Military Committee were ‘Alawis, members of a heterodox Shi‘i Muslim sect, which, as a group, was historically the least-favored rural community in Syria. Like Hafiz al-Asad, ‘Alawi boys who could get to secondary school often chose the military academy as a promising start for social advancement. Once in power, Asad naturally selected trusted men—brothers, cousins, clansmen in the first instance—for sensitive posts, and ‘Alawis have been prominent in the military and security services since the mid-1960s.¹⁸ That Syria is ruled by a man from a village in the ‘Alawi hill country and that the rural areas of Syria have for more than twenty years obtained a respectable share of the state’s resources are striking consequences, though surely not intended ones, of the Baath movement ‘Aflaq and Bitar started a half-century ago.

Baathism was originally brought to Iraq by students who were Shi‘is, and the party’s leaders in the 1950s were predominantly of that sect.¹⁹ Recruits to Baathism in Iraq, like recruits in Syria, Jordan, or other states, were attracted to this opposition movement by dislike of foreign influence on their government, a desire to break the power of a ruling elite perceived as oppressive, and a belief in pan-Arab nationalism. The Baathists joined Qasim’s first government in 1958 but soon fell out with him over his reliance on the Iraqi Communist party and his lack of enthusiasm for pan-Arabism. The party sponsored an effort to assassinate him in 1959; its failure can be attributed in part to a lack of discipline within the assassination squad. Saddam Husain made his first appearance as a Baathist militant in that operation. The Iraqi Baath party was badly disrupted by subsequent government repression. In time, the Baathists recovered, and on February 8, 1963, they took power in a bloody coup. Several hundred Communists were killed in street fighting with the party militia or in torture chambers.²⁰

The Baath regime that succeeded Qasim in February 1963 destroyed through internal quarrels its capacity to govern and was ousted by the Iraqi military after nine months. ‘Aflaq’s National Command appointed a provisional Regional Command of Iraqis whom ‘Aflaq had met in the early 1960s while they were

¹⁸ The army is by no means exclusively ‘Alawi. Jadid is the only member of that sect to hold the post of chief of staff. Asad’s defense minister and chief of staff are Sunnis; loyalty to him is the key requirement.

¹⁹ Sa’dun Hammadi, prime minister from March to September 1991, is the only survivor from that era. He was a member of the Regional Command in the 1950s.

²⁰ Hanna Batatu, *The Old Social Classes and the Revolutionary Movements of Iraq* (Princeton, N.J., 1978), 974–94. This massive and detailed study is essential for understanding the political dynamics of Iraq in the twentieth century.

outside Iraq. Its six members were Sunnis from the Baghdad–Mosul–Al-Qa'im triangle (the third town is where the Euphrates enters Iraq from Syria). Of the three from Takrit, two were to be Iraq's future leaders, Ahmad Hasan Bakr and Saddam Husain. At an Iraqi Regional Congress in 1966, while the party was still illegal, five other Sunnis from the same area were added; then four of the eleven were Takritis. Men from that town have dominated the councils of the Baath party in Iraq for nearly thirty years and the government for over two decades. Saddam Husain's dictatorship, exercised since 1979 through a tight circle of relatives and marital connections, stems from his attracting 'Aflaq's favorable notice during attendance at a party congress in Damascus in 1963.

On July 17 and 31, 1968, the rebuilt Baath party seized power from the military faction then ruling Iraq. The second date is that on which the Baathists ousted the non-Baath military faction that had provided the force essential to success on July 17. Earlier in 1968, 'Aflaq's followers in exile from Syria, together with Iraqis, Lebanese, and others, held a National (pan-Arab) Congress that formalized the split between them and the regionalists in Syria. Iraqis were prominent on the National Command that the congress chose. Alert to the advantages of party legitimacy that association with 'Aflaq would bring, Bakr and Saddam made sure this command stayed linked with Iraq. They offered the 'Aflaq group a haven in Baghdad, organized another National Congress in 1970, and ensured that 'Aflaq retained his post of secretary-general. After several lengthy visits to Iraq, he made his home permanently in Baghdad in 1975, when civil war made Beirut an unsuitable residence. Until his death in 1989, 'Aflaq was honored as the founder but had no influence on party affairs or government policy. Real power lay with Bakr and Saddam and their growing coterie of kinsmen from the Takrit district.

Once the Baathists had seized power, Bakr and Saddam worked as a team for a decade to assure permanent Baath control over Iraq. The older man (Bakr was born in 1914, Saddam in 1937) had impeccable credentials as one of the Free Officers who overthrew the monarchy in 1958. Aware that the Iraqi officer corps' belief in its right to be part of the political process posed a danger to their dominance, Bakr and Saddam forced influential Baathist officers out of office by 1973. Three years later, Bakr arranged for Saddam, who had never served in the military, to be commissioned as a lieutenant-general; Saddam made himself field marshal in 1979. Saddam, meanwhile, laid his own plans. Through the 1970s, he systematically purged potential rivals and promoted supporters, employing multiple security services accountable to him personally. Several years as head of the party's security arm enabled him to choose for these services leaders sufficiently ruthless to carry out any order. In mid-1979, he pushed Bakr into a retirement that amounted to virtual house arrest, discovered a "plot" against himself, and executed a score of colleagues, including five on the Regional Command. His control of all instruments of government was complete.

The scope of Saddam's agenda became abundantly clear once he could drop the pose of deferential second-in-command to Bakr. His insatiable ambition for power and the adulation that accompanies it is manifest in the thousands upon thousands of representations of him throughout the country, the dozens of public works named for him, and the huge monument to "victory" over Iran which

features bronze replicas of his “forearms and fists, sixteen metres in length . . . [rising] with their firmly grasped swords to an apex forty metres above the ground.”²¹ Externally, he moved to take advantage of Egypt’s isolation in the Arab world following its signing of a peace treaty with Israel in 1979. Two major miscalculations have thwarted his ambition to have Iraq supplant Egypt as the leading Arab state. Iraq’s invasion of Iran in September 1980 led to eight years of debilitating warfare, and its seizure of Kuwait in 1990 has shattered his leadership image and weakened his hold on power.

UNDER THE AUTHORITARIAN RULE of the past two decades, the Syrian and Iraqi Baath organizations have grown enormously. As of the mid-1980s, the parties have 100,000 full members and 400,000 candidates in Syria, 25,000 full members and 1.5 million candidates in Iraq. Over the same period, however, vitality has drained out of each. People join for party benefits—jobs, access to places in the university—or merely to avoid suspicion of disloyalty. Party meetings and publications are devoted to indoctrination of members with the regimes’ attitudes and policies. Neither leader has any use for an organization that allows members freely to propose new ideas, criticize regime policies, or suggest that the leader can make mistakes. Baathism plays little part in either leader’s Arab policy; each uses Baath rhetoric but deals externally through the Arab League, other regional groupings, or bilaterally. The most recent National (pan-Arab) Congress of the party held by the Baghdad-based Baath was in 1977 and by the Damascus-based Baath in 1980.

Asad’s tenure in Syria divides, very broadly, into three phases. In the earliest, he cemented the loyalty of the rural population by devoting resources to land reclamation, education, and services such as electricity and water, in addition to allowing a significant measure of private economic activity.²² Next, for half a dozen years, the regime fought an Islamic revolutionary movement that sprang from the urban Sunni Muslim groups the Baath had replaced, in a combat conducted with increasing ferocity on both sides. The government broke the power of the Islamists with extremely harsh measures in Hamah in 1982. Not long afterward, Asad suffered a serious illness, which has diminished his ability to govern efficiently. He lacks the energy to attend to detail but continues his career-long practice of resisting delegation of authority. Combined with the heavy cost of a military build-up to confront Israel, this diminution of leadership has resulted in a sense of drift domestically and in negative economic growth in the late 1980s. A stagnating dictatorship has been Syria’s lot for nearly a decade.

If Asad can rightly be assessed as the man who converted Baathism in Syria from party rule into a dictatorship, Saddam Husain can be characterized as the man who turned the Iraqi Baath system into tyranny. His first public act as a

²¹ Samir al-Khalil, *The Monument: Art, Vulgarly, and Responsibility in Iraq* (Berkeley, Calif., 1991), 3.

²² Raymond A. Hinnebusch, in *Peasant and Bureaucracy in Ba’thist Syria: The Political Economy of Rural Development* (Boulder, Colo., 1989); and *Authoritarian Power and State Formation in Ba’thist Syria: Army, Party, and Peasant* (Boulder, 1990), analyzes the bases of Asad’s system and the rise of a new elite.

Baathist militant found him, gun in hand, attempting to assassinate a prime minister. Force has been his preferred tool to accomplish ends, whether internally or externally. Both regimes have poor human rights records, routinely using torture on detainees; Iraq's is by far the more extensive. Close colleagues whom Saddam suspects of disloyalty or even holding views differing from his have been executed. Samir al-Khalil, in his chilling analysis of Saddam's system, lists a number of the more prominent victims.²³ He also argues, persuasively, that Saddam has worked to alienate Iraqis from their traditional societal groups and force them into total reliance on the state, which governs through doling out rewards and punishments and routinely uses institutionalized violence.

Khalil also contends passionately that Saddam's creation of a system in which citizens come to believe that they share responsibility for actions over which they have no say derives from Baath ideology. Perhaps, but Saddam's personal rule goes far beyond anything that Baathist ideologues conceived. His system is totalitarianism carried to the ultimate degree, if that is not an overly tautological phrase. In pursuit of his personal goals, Saddam has abandoned pan-Arabism, presented himself as a Muslim leader (the Baath is an areligious ideology), and claimed direct descent from the Prophet Muhammad.²⁴ His inner circle of associates has become progressively dominated by men related to him by blood or marriage. The Baath organization serves as both rubber stamp and propaganda channel. The primary function of the party member has become one of support for Saddam's compulsion to retain and exercise power.

²³ Samir al-Khalil, *Republic of Fear: The Politics of Modern Iraq* (Berkeley, Calif., 1989), 291–96. Two other useful works are Committee against Repression and for Democratic Rights in Iraq (CARDRI), *Saddam's Iraq: Revolution or Reaction?* (London, 1986); and Marion Farouk-Sluglett and Peter Sluglett, *Iraq since 1958: From Revolution to Dictatorship* (London, 1987). Saddam's life is covered in Efraim Karsh and Inari Rautsi, *Saddam Hussein: A Political Biography* (New York, 1991).

²⁴ In an interview, Saddam said, "I no longer view Arab unity . . . as I did in the past . . . We (Arabs) in actual fact are several entities"; *Al-Tadamun* (London), February 6, 1988, translated in Foreign Broadcast Information Service/NES, February 12, 1988. Saddam's portrayal of "himself as a blood descendant of the Prophet . . . is a rather extraordinary claim"; Mary Jane Deeb, *Washington Post*, February 2, 1991.

The Historiography of Modern Iraq

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THE COMPILATION OF A CRITICAL BIBLIOGRAPHY of modern Iraqi history is not a particularly arduous task, although the relative paucity of scholarly studies is neither an accurate reflection of the intrinsic interest of the subject nor of the quality of much that has been produced. The best writing on modern Iraq ranks among the best writing on modern Middle Eastern history, and much recent scholarship, including the work of younger Iraqi scholars like 'Isam al-Khafaji, Hadi al-'Alawi, Faliḥ 'Abd al-Jabbar and 'Abd al-Salam Yusuf, has concentrated on analytical issues such as the nature of the *rentier* state, the interpretation and significance of modern Islamic thought, and forms of cultural discourse, all concerns widely shared by their contemporaries in other Arab countries.¹ Among the more tragic aspects of the brutal and despotic regimes under which the population of Iraq has suffered for so long is that many of its most talented citizens have been forced into exile and that the best of its scholars can only express themselves freely outside Iraq.

Apart from this, an important reason for the lack of academic attention Iraq has received is the sheer difficulty of carrying out research there, for Iraqis and foreigners alike. Thus the best-known field studies of modern Iraqi society² are based on work carried out well before the Revolution of 1958; no comparable studies have been published since then, although a number of Iraqis have produced doctoral theses on sociological or anthropological topics that have tended to remain confined to the shelves of British or American university libraries (that is, they have not been published in book or article form in any European languages). This is also the case for less obviously sensitive historical periods; as far as is known to us, only one foreign scholar has carried out the kind of research in the Islamic law courts or the local Ottoman archives comparable to that of Abd al-Karim Rafeq or André Raymond and their students, which has so illuminated our understanding of Syria and Egypt in the seventeenth to nine-

¹ See Hadi al-'Alawi, *Fi al-din wa-al-turakh* [On Religion and Heritage] (Beirut, 1973); *Fi al-siyasah al-Islamiyah* [On Islamic Politics] (Beirut, 1974); *al-Mu'jam al-'Arabi al-jadid* [The New Arabic Lexicon] (Latakia, 1983); compare 'Isam Khafaji, *al-Dawlah wa-al-Tatawwur al-Rasmali fi al-'Iraq, 1968–1978* [Capitalist Development in Iraq 1968–1978] (Cairo, 1983). There is a summary and review of Khafaji's book by Marion Farouk-Sluglett in *MERIP Reports* (Middle East Research Information Project), nos. 125–26 (July–September 1984): 51–52.

² Robert A. Fernea, *Shaykh and Effendi: Changing Patterns of Authority among the El Shabana of Southern Iraq* (Cambridge, Mass., 1970); Edmund Ronald Leach, *Social and Economic Organisation of the Rowanduz Kurds* (London, 1940); Shakir M. Salim, *Marsh Dwellers of the Euphrates Delta* (London, 1962).

teenth centuries. It will be of great interest to see how Dina Rizk Khoury's work on this material develops further in the future; in the meantime, her article on commercial agriculture in the province of Mosul at the end of the eighteenth century is a hopeful harbinger of work to come.³

As far as more recent history is concerned, the majority of writing about Iraq in European languages, and much of the more serious writing in Arabic, is written by scholars who cannot or do not go there and is based primarily on European or American archives, newspapers, Iraqi and international official statistics, and published broadcast material. It is perfectly possible to write sound diplomatic and political history from such sources, but its limitations for any other purpose are obvious enough. More insidious is that, until recently, much that has purported to be scholarly writing on Iraq, including books that still find their way into university reading lists, consists either of uncritical apologia for present or past regimes or records the "official version" of events, presented by the researcher as if it were historical fact.

Because none of the governments of modern Iraq (including the monarchy) have permitted the kind of academic freedom in which original and innovative scholarship could flourish, little serious work on modern history has been carried out at universities within the country. Indigenous Iraqi scholarship in this field has therefore been dominated by intellectuals from the various dissident political parties. Apart from the writings of the Shi'i movement in recent years, especially those of Ayatullah Baqir al-Sadr and his followers, most of this has been undertaken by orthodox Marxists.

These intellectuals have published extensively in such journals as *al-Thaqafa al-Jadida*, or *al-Nahj*, produced in Cyprus and Damascus. Many have long been living in exile, with the result that debates on themes such as the role of the state, land reform, the Kurdish question, the Shi'i movement, or the emergence of new social classes have not taken place in conventional academic forums but among politically committed intellectuals. Apart from the memoirs of former ministers and other politicians and the work of "chroniclers" such as 'Abd al-Razzaq al-Hasani, the author of a ten-volume *History of the Iraqi Cabinets* in Arabic,⁴ there is little in the way of scholarly publication on the modern period within Iraq itself.

ANY CONSIDERATION OF MODERN IRAQI HISTORY must begin with Hanna Batatu's *Old Social Classes and the Revolutionary Movements of Iraq: A Study of Iraq's Old Landed and Commercial Classes and of Its Communists, Ba'thists, and Free Officers* (1978), of which one reviewer has written: "Hanna Batatu has constructed a masterpiece of historical literature that singlehandedly catapults Iraq from the least known of the major Arab countries to the Arab society of which we now have the most thorough political portrait."⁵ The book, really three books in one, is the result of twenty

³ Dina Rizk Khoury, "The Introduction of Commercial Agriculture in the Province of Mosul and Its Effects on the Peasantry 1750–1850," in Çağlar Keyder and Faruk Tabak, eds., *Landholding and Commercial Agriculture in the Middle East* (Albany, 1991), 155–71.

⁴ 'Abd al-Razzaq al-Hasani, *Tarikh al-Wazarat al-Iraqiyah*, 10 vols. (Sayda, 1953–61).

⁵ Joe Stork, *MERIP Reports* (June 1981): 23.

years of research; it is a work of rigorous scholarship and passionate commitment. The first part, covering the social and political history of the late nineteenth and early twentieth centuries, deals with the old social classes and the origins of the modern Iraqi state, based largely on an extremely careful review of the secondary literature and British consular and diplomatic reports.

The second part of the book is concerned with the history and development of the Iraqi Communist party up to the middle 1950s, based on a mass of documentary material, much of it taken, somewhat in the manner of Richard Cobb for eighteenth-century France,⁶ from police files but also on numerous interviews with people from almost all political backgrounds. Some of the descriptions of the famous demonstrations of the late 1940s are breath-taking in their authentic portrayal of the mood and spirit of the time. In addition to the quality of the analysis, the minuteness of the documentation, the extensive tables, and the precision of the narrative make the book itself an indispensable source for the period.

The third and final section, perhaps the least satisfactory part of the book, deals with the period from the middle 1950s until the early 1970s, ranging from the background and execution of the Iraqi Revolution of July 1958 to an account of the seizure of power by the present regime in July 1968 and its consolidation over the next four or five years. The influences on Iraq of the political conjunctures in the rest of the Arab world, the idiosyncratic role of the Communist left in the immediately pre and postrevolutionary periods, the nature of pan-Arabism and the way in which the Baath party was able to seize and maintain the political initiative in spite of its very narrow political base are all carefully analyzed. Very occasionally, the book's unique depth and detail can be overwhelming, but the style is clear and uncluttered and the narrative lively.

The impact of Batatu's book on the historiography of the modern Middle East in general and of modern Iraq in particular has been immense, including but extending far beyond Batatu's own students at the American University of Beirut and at Georgetown. Early in 1989, the University of Texas at Austin convened a conference whose theme was a reexamination of the Revolution of 1958 both in the light of the opening of the British archives for 1958 and of Batatu's analysis of the events surrounding the revolution. The title of the collection that resulted, *The Iraqi Revolution of 1958: The Old Social Classes Revisited* (1991), reflects these twin concerns. Apart from a number of fascinating eyewitness accounts of the events of 1958, many of the contributions to the book raise themes of more general concern. These include the question of the cultural hegemony of the left in the 1950s (Abd al-Salam Yousif), the functioning of state institutions and decision-making processes—"putting the state back in"—(Roger Owen) and community, class, and minority politics (Sami Zubaida).

IN WHAT FOLLOWS, we will discuss a number of topics we consider crucial for an understanding of contemporary Iraq, including some that are only rarely

⁶ Richard Cobb, *The Police and the People: French Popular Protest, 1789–1820* (Oxford, 1970).

reflected in the literature, especially in the more conventional academic accounts of modern Iraqi history and politics. One of the many unresolved questions concerns the extent to which market relations had or had not transformed society by the time of the Revolution of 1958.⁷ It seems that the penetration of money relations in much of urban and rural society during the first half of the twentieth century took place largely in the sphere of exchange and did not fundamentally transform the forms of production; while local and international commerce expanded, commodities continued to be produced under preindustrial conditions. As a result, both in town and countryside, production was largely organized within the household or extended family unit. Hence, although many millions of Iraqis were profoundly affected by the changes in the framework of their lives in the middle years of the twentieth century, much of society was still rooted in preindustrial ways of life and practice, and in this respect Iraq remained somewhat less developed than Egypt, Syria, or Lebanon.

For this reason, while the student of Iraq finds many of the questions, themes, and controversies arising from the study of the modern Arab world of interest and concern, Iraq does differ more substantially from its Arab neighbors on a variety of levels than has often been acknowledged. Throughout much of Iraqi society, the significance on a day-to-day level of an individual's regional background, family, clan, and tribal affiliation continues to be much more pronounced than in the long-established, settled urban and rural societies in some of the neighboring states. Thus, while much has been made, and correctly so, of the instrumentality of British policies in creating a small class of powerful, semi-feudal landlords and the virtual enslavement of the tribal population that accompanied the process, the wider socioeconomic implications this has had for the formation of modern Iraqi society have often been overlooked.

Of course, any attempt to trace processes of class formation in most of the Third World is somewhat daunting. In Iraq, the persistence of pre-modern value systems and social norms into modern times makes it particularly difficult to determine how far one can really speak in terms of social classes. In the first place, while presumably intending to promote the formation of an integrated nation-state, the governments under the mandate and monarchy (1920–1958) in fact helped to reconstruct and perpetuate pre-capitalist and tribal relations through their tribal and land tenure policies. In essence, these policies served to arrest the decline and disintegration in the power of tribal leaders that was visibly taking place at the end of the Ottoman period⁸ by giving administrative and fiscal powers, and land grants, to selected shaykhs to enable them to tax and control those who subsequently became “their” tribesmen; as one of Robert Fernea's older informants told him, “in those [Ottoman] days it was dangerous for the

⁷ Marion Farouk-Sluglett and Peter Sluglett, “The Transformation of Land Tenure and Rural Social Structures in Central and Southern Iraq, c. 1870–1958,” *International Journal of Middle East Studies*, 15 (1983): 491–505; *Iraq since 1958: From Revolution to Dictatorship*, 2d edn. (London, 1990); “The Social Classes and the Origins of the Revolution,” in Robert A. Fernea and William Roger Louis, eds., *The Iraqi Revolution of 1958: The Old Social Classes Revisited* (London, 1991), 118–41; Hanna Batatu, “The Old Social Classes Revisited,” in *ibid.*, 211–22.

⁸ Farouk-Sluglett and Sluglett, “Transformation of Land Tenure”; Fernea, *Shaykh and Effendi*; Peter Sluglett, *Britain in Iraq, 1914–1932* (London, 1976), 231–58.

shaykh to have large numbers of tribesmen angry with him.”⁹ To some extent therefore, such transformations as did take place before 1958 occurred in spite of rather than because of policies pursued under the mandate and monarchy.

In the postrevolutionary period, there were two key developments. First, migration continued to increase, both relatively and absolutely, and, as a result, while 35 percent of the population lived in the cities in 1947, the figure had risen to 65 percent by 1977. Second, the various land reforms and the spread of the writ of the state to almost all parts of the country meant the gradual incorporation of the whole of rural Iraq into the state proper and the national market. Such developments, it might reasonably have been assumed, should have led to a fairly rapid transition from the values of *Gemeinschaft* to those of *Gesellschaft*, a fundamental precondition for the development of the nation-state. A second important theme that follows, therefore, is the extent to which this transition has not occurred.

While change has evidently taken place on a number of important levels, much has happened only on the surface—modern consumerism and sophisticated communications systems, for instance—and constitutes a facade of modernity rather than a genuine transformation of pre-capitalist forms of production and value systems. Beneath this facade, patriarchal values, and ties of family, clan, locality, tribe, and sect continue to be reproduced, since the existence of a highly dictatorial and repressive regime for more than two decades has operated against their disintegration. As Zubaida puts it, “The ‘orientalist’ picture of ‘Islamic’ societies as communalistic, religious and impervious to modern ideologies has actually been realized as a modern phenomenon under totalitarian regimes in Iraq and elsewhere.”¹⁰ A particularly visible expression of this is the degree to which the present regime (the closely knit group around Saddam Husain) bases its own rule almost exclusively on family, clan, and regional alliances.

The importance of taking these factors into account becomes clear when reviewing much of the recent writing on modern Iraq. By any objective standard, what is one of the most appalling dictatorships in the Third World¹¹ has been presented, either directly or by implication, as a kind of necessary evil that a society as “heterogeneous” as Iraq somehow “requires,” the anvil on which the nation-state has to be hammered out. Such “analysis” misunderstands the retrogressive implications of this kind of political system for “nation formation,” whether in Iraq or anywhere else, and, in its more extreme forms, almost becomes an apologia for the regime’s excesses. It is sometimes accompanied by the assertion that Iraq can only be governed by a strong man,¹² a myth to which some Iraqis unfortunately also subscribe.

The notion of the heterogeneity of Iraqi society is another theme that needs further definition and refinement. The facts are that the population of Iraq, now about 18 million, is divided on both ethnic and sectarian lines. Of course, neither

⁹ Fernea, *Shaykh and Effendi*, 121.

¹⁰ Sami Zubaida, “Community, Class and Minorities in Iraqi Politics,” in Fernea and Louis, *Iraqi Revolution of 1958*, 209.

¹¹ David A. Korn (Middle East Watch), *Human Rights in Iraq* (New York, 1990).

¹² This theme is picked up in Elie Kedourie, “The Kingdom of Iraq: A Retrospect,” in Elie Kedourie, *The Chatham House Version, and Other Middle Eastern Studies* (London, 1970), 236–82.

the communities nor the sects constitute homogeneous or monolithic single entities. As far as it is possible to make any general calculations (since only primary religious affiliation, that is "Christian" or "Muslim," is recorded in Iraqi censuses), some 72 percent are Arabs, about 22 percent Kurds, and the remainder Turcomans, Armenians, and others. Muslims make up 95 percent; the remainder are Christians and members of various heterodox sects. Almost all Kurds (apart from the Yazidis), and all but a tiny minority of Arabs (who are Christians), are Muslims; to that extent, therefore, there is a degree of homogeneity arising from the fact that some 70 percent of the population is both Muslim and Arab.

As far as the Kurds are concerned, their status and, in general, their political aspirations are primarily defined by considerations of language and ethnicity. Kurdish politics since the formation of the state of Iraq have generally focused on the quest either for a "Grand Kurdistan" embracing the Kurdish population of the entire area or, more realistically, for some kind of autonomous status for the Kurdish area within the state of Iraq, either federated or in some other close relationship with Baghdad. Almost all Kurds are Sunni Muslims, but in practice their membership in the same sect does not form a link binding them to the Sunni Arabs.

Between two-thirds and three-quarters of the Arab population (of some 14 million) are Shi'is; thus there are 9 to 10 million Shi'i Arabs and 4 to 5 million Sunni Arabs. Apart from Baghdad, which contains mixed Sunni and Shi'i quarters as well as quarters with particularly strong Shi'i or Sunni representation, it is broadly true to say that Sunni and Shi'i Arabs inhabit distinct parts of the country. Historically, the Sunni Arabs constituted the majority of the urban population and were always politically dominant under the monarchy and the republic.

The main reason for this was that the Ottoman empire was a Sunni institution and tended to employ only Sunnis in the administration. In addition, when modern state education first appeared in Iraq at the end of the nineteenth century, few if any Shi'is attended the new state schools. Consequently, when the Iraqi state was created in 1920, there were few qualified Shi'is able or willing to take part either in the leadership of the government or in the administration, and this situation continued with regard to cabinet participation throughout the monarchy and with regard to positions of power under the republic. However, Shi'i tribal leaders were among the richest landowners and Shi'i businessmen among the richest merchants by the time of the Revolution of 1958, and the rapid spread of educational provision produced increasing numbers of qualified Shi'is. Thus by the middle 1950s, the Shi'i-Sunni divide had begun to assume less significance. In addition, the quest for national independence in the 1930s, 1940s, and 1950s had a generally unifying effect, a tendency that continued for some years in spite of the deep political divisions that emerged after the revolution. Finally, the secular atmosphere and the general sense of optimism in Iraq and the Middle East as a whole during the 1950s and early 1960s gave rise to the hope that the significance of sectarian divisions would gradually disappear.

The optimism of the 1950s and 1960s seems far away in the early 1990s. A simplistic image of Iraqi society has emerged, largely under the influence of the Middle Eastern "experts" of the U.S. defense establishment, of "the Arab Sunnis"

supporting the "Sunni" regime of Saddam Husain and the allegedly "somewhat less Arab" Shi'is (a sort of Iranian fifth column) bitterly opposed to it, with the Kurds in another category altogether. It is certainly the case that the combination of the general revival of Islamic sentiment, the Iranian Revolution, and the Iran-Iraq War have all contributed to the reemergence and reassertion of sectarian feeling, but far more crucial than these factors has been the persistence and revival of the kind of factional loyalties to tribe, clan, region, and family described earlier.

Almost paradoxically, the persistence of such loyalties has been a major contributing factor to "the revival of Islam," as far as this has manifested itself in Iraq, as well as to the reinforcement both of sectarianism and of the so-called heterogeneity of Iraqi society. "Islamic revival" has been encouraged partly because the reassertion of these loyalties encompasses and includes the promotion of ways of life and value systems embedded in Arab-Islamic consciousness. Sectarianism, localism, and other forms of communal solidarity were reinforced largely because the powerlessness of the individual vis-à-vis an arbitrary political system has had the effect of forcing men and women to resort to "pre-state" networks of sect, locality, or family. In Iraqi terms, using such networks means strengthening loyalties to a particular Shi'i or Sunni (or Kurdish or Christian) tribe, region, or family.

In this way, the contemporary political system has acted as one of the main causes of the reproduction of heterogeneity rather than contributing to its demise. Support for the present regime rests with the Takriti clans and long-standing individual associates of Saddam Husain, and cannot simply be represented *a priori* as Sunni support for a Sunni government. It is possible, however, that broad sections of the Sunni population which might otherwise not have supported Saddam Husain may well have done so out of fear of the advent of a Shi'i fundamentalist regime. At the same time, many secular-minded Shi'is outside the Holy Cities of Karbala' and Najaf probably shared such fears and decided to support the regime for similar reasons.

Finally, the notion of "nationalism," the controversies surrounding it and its precise meaning at different historical periods, has always been a vexed question. Much of the confusion has arisen from a certain looseness of terminology, in which the different but overlapping notions of "patriotism," "Iraqi nationalism," and "pan-Arab nationalism" have all been assimilated into the single word "nationalism," which posits a greater degree of coherence and continuity within nationalist thinking than actually existed. There is very little ideological linkage between the political aims of figures like Yasin al-Hashimi¹³ in the 1920s and 1930s, 'Abd al-Salam 'Arif¹⁴ in the 1950s and 1960s, and Saddam Husain.

¹³ Yasin al-Hashimi, a leading Arab nationalist politician of the 1920s and 1930s, was born in Baghdad in 1884. He became a high-ranking Ottoman military commander in World War I and was subsequently appointed chief of Faisal's general staff in Damascus. In 1922, he returned to Iraq and became minister of defense. He was prime minister in 1924–25 and 1935–36 and died in exile in 1937. He embodied an essentially "Iraqist" Arab nationalism and was fairly consistent in his support for attempts to reduce British influence in Iraq. See Phebe Marr, "Yasin al-Hashimi: The Rise and Fall of a Nationalist: A Study of the Nationalist Leadership in Iraq 1920–1936" (Ph.D. dissertation, Harvard University, 1967).

¹⁴ 'Abd al-Salam 'Arif (1921–1966), a fervent pan-Arab nationalist and supporter and admirer of

In addition, this kind of analysis has tended to concentrate on the behavior and thinking of a fairly narrow political elite and ignores the wider political culture emerging in the 1940s and 1950s among the expanding urban middle, lower middle, and working classes.¹⁵ Substantially influenced by the Communist party, this political culture was dominated by the desire to ameliorate or eradicate poverty and to realize greater social justice. As it was widely believed that genuine social reform could not be carried out until Iraq was fully independent from Britain, those who believed in these ideals considered the struggle for social justice and the struggle for national independence to be inseparable. Hence, although it is true that national independence was the goal of almost all politically conscious Iraqis, only a small minority of those who espoused these sentiments were "pan-Arab nationalists" in the strict sense of being in favor of merging Iraq into a larger Arab entity.

In general, until the Suez crisis and the tripartite invasion of Egypt in 1956, the appeal of pan-Arab nationalism in Iraq was largely confined to sections of the Sunni Arab urban middle and lower middle classes, for two main reasons. In the first place, pan-Arab nationalism has always been a predominantly urban phenomenon, and in the 1940s and 1950s, the Sunnis formed the majority of the urban population. Second, the Arab world outside Iraq is overwhelmingly Sunni, while over one-quarter of all Iraqis are Kurds and more than half are Shi'is. Although, as we have already noted, the two communities are not monolithic, it is generally the case that neither has seen its interests being best served by Iraq joining a wider Arab federation.¹⁶ No major Nasserist political organization was ever set up in Iraq, and even though an Iraqi branch of the pan-Arab Baath party was founded in 1951, its appeal was limited, and party membership remained extremely small.¹⁷ Thus pan-Arab nationalism was projected backward onto Iraq by the Baath after 1968, and history was to some extent rewritten to extend the historic role both of pan-Arab nationalism and of the Baath itself.

President Nasser of Egypt, was president of Iraq between 1963 and 1966, when he was killed in a helicopter accident. A professional soldier, he had been a member of the supreme committee of the Free Officers in 1957–1958 and thus one of the guiding spirits behind the Revolution of 1958. After the revolution, he was appointed minister of interior and deputy commander of the armed forces but soon fell out with Qasim because of his espousal of Nasserism and, more concretely, his desire to take Iraq into the United Arab Republic of Egypt and Syria in the autumn of 1958. He attempted to kill Qasim in November 1958, was tried, sentenced to death but pardoned, and released from prison in October 1962. He was the leader of the Nasserist/Baathist group of officers that overthrew Qasim in February 1963, and he was Qasim's immediate successor as president. See Hanna Batatu, *The Old Social Classes and the Revolutionary Movements of Iraq: A Study of Iraq's Old Landed and Commercial Classes and of Its Communists, Ba'athists, and Free Officers* (Princeton, N.J., 1978), 767–872 *et passim*, 1003–70 *et passim*; Farouk-Sluglett and Sluglett, *Iraq since 1958*, 48–60, 93–111.

¹⁵ See Phebe Marr, *The Modern History of Iraq* (Boulder, Colo., 1985); Majid Khadduri, *Independent Iraq 1932–1958: A Study in Iraqi Politics*, 2d edn. (London, 1960); Khadduri, *Republican Iraq: A Study in Iraqi Politics since the Revolution of 1958* (London, 1969).

¹⁶ It is often pointed out that many of the first Baathists in Iraq were Shi'is. This was largely a coincidence, in that many were friends or relatives of the first secretary-general of the party, Fu'ad al-Rikabi, a Shi'i engineer from Nasiriyyah. No Shi'is have held positions of real power in the upper echelons of the Baath party since 1963.

¹⁷ See Marion Farouk-Sluglett and Peter Sluglett, "The Iraqi Ba'ath Party," in Vicky Randall, ed., *Political Parties in the Third World* (London, 1988), 57–74. According to Batatu, *Old Social Classes*, 808, police records show that the party had 289 members in 1955.

A DIFFERENT AND HIGHLY ORIGINAL ANALYSIS of pan-Arab nationalism and contemporary Iraqi history is provided in *Republic of Fear: Saddam's Iraq* (1989, 1990) by Samir al-Khalil, the pseudonym of a well-known Iraqi architect and publisher who had great difficulty in getting his original manuscript accepted for publication. It is a brilliantly savage denunciation and analysis of Arab populist politics that shows how Baathist pan-Arabism was developed into an all-embracing instrument of state terror, how Saddam Husain and his circle perfected it for this purpose, and how far this process succeeded in debasing public discourse and political life in both Iraq and the Arab world.

Briefly, as developed by its ideologues in Syria in the 1940s, Baathism is a form of populist pan-Arabism asserting the historic unity of the "Arab nation": only when the Arab world, artificially divided first into provinces by the Ottomans and subsequently into separate states by imperialism and Zionism, is reunited, can the "Arab nation" fulfill its "eternal mission" (*risala khalida*). Much of Baathist rhetoric consists of vague assertions of this kind, with references to an idealized and romanticized notion of the past; there is very little indication of how power should be won or how it should be wielded later. After 1968, this ideology was used to legitimate the notion that the Baath were ruling Iraq in the "interests of the Arab nation." Those who opposed it or questioned its authority or legitimacy were by definition "enemies of the Arab nation," traitors who condemned themselves by their own statements or actions.

This kind of thinking was systematically diffused through schoolbooks, the media, and the Baath party and its mass organizations. In 1978, membership in any other political party became a capital offense for members or ex-members of the armed forces (there is universal conscription in Iraq); any political discourse that did not endorse or applaud Baath rule became first impossible and then dangerous. All this was accompanied by the growth of a degree of repression and lawlessness almost unparalleled in the Arab world and a vast personality cult around Saddam Husain. In time, as al-Khalil shows, the Iraqi population became so mesmerized by the constant barrage of propaganda directed toward it that people stopped even thinking anything controversial. Al-Khalil does not directly address himself to the wider Middle Eastern situation or Iraq's role within it, one of the few criticisms that can be made of the book. Nevertheless, it is refreshing to find an author, writing well before it was fashionable or advisable to demonize Saddam Husain, with the courage and honesty to give an accurate description of this utterly repressive regime. *Republic of Fear* is a pessimistic and deeply troubling book, but it has the merits of candor and realism.

It might be difficult for the uninitiated reader, turning from *Republic of Fear* to some of the more conventional academic offerings, to grasp that both are discussing the same country. Majid Khadduri's *Socialist Iraq: A Study in Iraqi Politics since 1968* appeared in 1978, at the end of the first decade of Baathist rule, admittedly before Iraqi politics reached the crescendo of horror and repression of the late 1980s, but it still paints an undeservedly rosy and somewhat less than frank picture of the political system and the political elite. This tendency is even more marked in Khadduri's *Gulf War: The Origins and Implications of the Iraq-Iran Conflict* (1988), a paean of praise to Saddam Husain that it can only be hoped a

combination of good taste and changes in recent perceptions of Iraq will push quietly into obscurity. There are several problems common to both *Socialist Iraq* and Khadduri's *Republican Iraq: A Study in Iraqi Politics since the Revolution of 1958* (1969) but not, mercifully, to his best book, *Independent Iraq, 1932–1958: A Study in Iraqi Politics* (second edition, 1960), which remains a useful and well-informed survey of the events of that period. First, none of the books contains a bibliography or footnotes; second, the latter two books rely heavily on official publications and interviews. Since the interviews are normally with members of the regime of the day rather than the opposition, a certain bias is inevitable. Institutions such as the provisional constitution and the Revolutionary Command Council are solemnly described in such a way as to suggest to the uninitiated that they had some real relevance to the way in which the country was run. But the provisional nature of the constitution is such that it has been suspended since 1958; also, while the council was nominally the country's supreme executive body and its members elected by the Baath party, all important decision making and politics took place among an informal circle of individuals that was already dominated by Saddam Husain in the late 1970s and that also oversaw all appointments to the council's ranks.

Again, although Khadduri might claim that his own craft, political science, precludes him from forays into economic and social analysis, the virtual absence of this dimension makes much of the political conflict he describes almost incomprehensible. Even in his capacity as a political scientist, he does not attempt to explain what Baathism actually is in practice or what "socialism" means or meant to the men who rule Iraq. Two quotations give the general atmosphere: "the leadership has been able to maintain on the whole a high degree of stability and continuity by applying various measures of conformity, including disciplinary action"; "More important perhaps are [Saddam Husain's] potentials in prudence, flexibility and resourcefulness . . . These qualities, combined with integrity and high moral courage, are his Party's best promise for the country's future leadership."¹⁸

Another example of this genre is *Iraq: Eastern Flank of the Arab World*, by Christine Moss Helms and published under the auspices of the Brookings Institution in 1984. Like Khadduri, Helms has benefited from interviews with senior members of the government and has tended to reproduce, sometimes rather uncritically, what she has been told. This comes out in accounts of the rise, structure, and function of the Baath party and especially in the description of the seizure of power or the attempted seizure of power by members of particular Baath factions as if these factions actually represented distinct ideological tendencies.

While it is true that the various struggles for power within the party leadership were couched in such terms, apparent ideological commitments were actually commitments to particular power groups and thus constituted the form rather than the substance of what was happening. What we have written elsewhere still applies; "as well as investing an essentially sordid and violent series of events with

¹⁸ Khadduri, *Socialist Iraq*, 41, 73, 76.

a certain dignity, this characterization obscures rather than clarifies the real nature of what was taking place, as if a historian of inter-war Chicago was to attempt to explain the interaction between Capone and his rivals in terms of competing political theories.”¹⁹

Similarly, it is instructive to compare Helms’s brief accounts of the events at Mosul and Kirkuk in March and July 1959 with Batatu’s careful investigation of the same incidents, published six years earlier.²⁰ Both were the scenes of bloody clashes between contending political and ethnic groups in which hundreds of people were killed. The underlying causes of these events and the precise identity of those who perpetrated them have been highly controversial. Helms simply repeats the official Baath version, in which the Communists are presented as subhuman monsters, and ignores Batatu’s carefully balanced and convincing study, which describes the background in great depth and the evolution of developments in minute detail.

Without wishing to belabor the point, the kind of writing that has just been described—and there are other examples²¹—is an unfortunate consequence of the desire of foreign policy and other establishments to be reassured, or to have pleasant things said, about regimes with which they are trading or otherwise interacting. The U.S. defense establishment is not alone; there was no chorus of criticism of the Iraqi regime’s anticommunist witch hunts from the Soviet Union, nor of its chemical weapons attacks on Kurdish villages in March 1988. And we should remember that, after the Iranian Revolution, Iraq was the second largest market (Saudi Arabia was the first) for the West and the Soviet Union in the Middle East, as well as being one of the world’s largest importers of arms; it was evidently important not to insult or offend key figures in that market and also to persuade doubters that such a regime was a worthy and responsible recipient for nuclear or chemical weapons technology. As one American commentator wrote at the end of 1990, “a fair amount of the current warmongering punditry is emanating from those who once had nothing but kind words to say about Saddam but who now loyally toe the administration’s line.”²² In the case of both Iraq under the Baath and Iran under the shah, the production of anodyne accounts about two fundamentally rather offensive regimes has backfired rather badly.

FOR THE KINDS OF REASONS ALREADY MENTIONED, historians’ knowledge of early modern Iraq and scholarly study of it are scanty. Stephen H. Longrigg’s somewhat arid *Four Centuries of Modern Iraq* (1925) is still the principal source in English for the Ottoman period, although it can be supplemented by the relevant

¹⁹ Farouk-Sluglett and Sluglett, *Iraq since 1958*, 134. Equally doubtful is the curious contention that ‘Ali Salih al-Sa’di and his gang of street fighters and assassins in 1963 “adopted such Marxist ideas as socialist planning, collective farms and workers’ control of the means of production”; Marr, *Modern History of Iraq*, 188.

²⁰ Christine Moss Helms, *Iraq: Eastern Flank of the Arab World* (Washington, D.C., 1984), 75; Batatu, *Old Social Classes*, 866–89.

²¹ See, for example, the articles by Peter Mansfield, Alya Sousa, and John Townsend in Tim Niblock, ed., *Iraq: The Contemporary State* (London, 1982).

²² Joel Beinin, *Middle East Report*, 168 (January–February 1991): 35.

chapters in P. M. Holt's *Egypt and the Fertile Crescent, 1516–1922: A Political History* (1966), Roger Owen's *Middle East in the World Economy, 1800–1914* (1981) and Charles Issawi's two collections of documents, *The Economic History of the Middle East 1800–1914* (1966) and *The Fertile Crescent 1800–1914: A Documentary Economic History* (1988). One of the few scholarly studies on the early nineteenth century is Tom Nieuwenhuis's *Politics and Society in Early Modern Iraq: Mamluk Pashas, Tribal Shayks and Local Rule between 1802 and 1831* (1982), an interesting examination of the various local tensions in the provinces of Baghdad and Basra in the period before the reassertion of Ottoman rule; it is particularly informative on the nature of tribal society during this period.

The very end of the Ottoman period and the British mandate are reasonably well covered, although some older books, such as Longrigg's *Iraq, 1900 to 1950* (1953), are no longer particularly useful. Stuart Cohen's *British Policy in Mesopotamia, 1903–1914* (1976), Ghassan R. 'Atiyah's *Iraq, 1908–1921; A Socio-Political Study* (1973), Helmut Mejcher's *Imperial Quest for Oil; Iraq 1910–1928* (1976), William Stivers's *Supremacy and Oil: Iraq, Turkey, and the Anglo-American World Order, 1918–1930* (1982), and Peter Sluglett's *Britain in Iraq, 1914–1932* (1976), with the various difference of emphasis that their titles suggest, are all based on British diplomatic and administrative documents, as is Philip Ireland's *Iraq: A Study in Political Development*, published in 1937; one minor mystery is how and why Ireland managed to gain access to key documents at least thirty years before they became more generally available. The various editions of Gertrude Bell's *Letters*,²³ Arnold Wilson's *Loyalties: Mesopotamia, 1914–1917* (1930) and *Mesopotamia, 1917–1920: A Clash of Loyalties* (1931), and the biography of Wilson by John Marlowe (*Late Victorian: The Life of Sir Arnold Talbot Wilson*, 1967) give fascinating accounts of the workings of two very different imperial minds.

The period between the end of the mandate and the Revolution of 1958 is discussed vividly in Batatu's *Old Social Classes*. Political events can be followed in Khadduri's *Independent Iraq* and the earlier chapters of general histories such as our *Iraq since 1958: From Revolution to Dictatorship* (1987, 1990), Phebe Marr's *Modern History of Iraq* (1985), which contains an annotated bibliography of the field until 1984, and Edith Penrose and E. F. Penrose's *Iraq: International Relations and National Development* (1978). The three latter books, especially Penrose and Penrose, in which oil is discussed extensively and with insight, have sections on the economy; a more detailed recent study is Joseph Sassoon's *Economic Policy in Iraq 1932–1950* (1987). Mohammad Tarbush's *Role of the Military in Politics: A Case Study of Iraq to 1941* (1982) deals with the formation and role of the Iraqi army, which played a crucial part in the period between the death of King Faisal I in 1933 and the "Anglo-Iraqi war" in 1941.

The historiography of the contemporary period, since the Revolution of 1958, and especially since the second Baath takeover in 1968, presents a bleak picture. Some of the difficulties of access and evidence already mentioned are even greater for the more recent past than for earlier periods, but, as has already been pointed

²³ Gertrude Lowthian Bell, *The Letters of Gertrude Bell*, Lady Bell [Florence E. E. (Olliffe) Bell], ed., 2 vols. (London, 1927); *Gertrude Bell; From Her Personal Papers, 1914–1926*, Elizabeth Burgoyne, ed. (London, 1961).

out, these are not the only problems. On the credit side, Uriel Dann's *Iraq under Qassem: A Political History, 1958–1963* (1969) is a factually accurate and reliable account of the period it describes, although it, too, is misleading over precisely what "nationalism" meant at different times and on Qasim's very ambiguous relations with the Communists. In general, the chapters on Iraq in the annual of the Tel Aviv Shiloah Institute, *Middle East Contemporary Survey* (1976–), mostly written by Ofra Bengio, are a mine of carefully collated and intelligently analyzed information from newspapers and news broadcasts. Again, Marr, the Penroses, and our own *Iraq since 1958* contain narrative histories and analyses of the events of the period up to their publication (respectively, 1985, 1978, and 1990), with useful sections on social and economic affairs.

Economy and society are also discussed extensively in the works of Batatu and al-Khalil already mentioned. An important article on the contemporary economy is Robert Springborg's "Infitah, Agrarian Transformation and Elite Consolidation in Contemporary Iraq," which raises interesting questions on the problems and prospects for economic liberalization within a *rentier* state and the roles of the public and private sectors. The discussion has been continued by Marion Farouk-Sluglett in "Irak: Rente pétrolière et concentration du pouvoir."²⁴

The work of Amatzia Baram is also illuminating for the modern period, in particular his attempt in *Culture, History, and Ideology in the Formation of Baathist Iraq, 1968–89* (1991) to trace the way in which the Baath has made use of the "Mesopotamian" heritage to construct a unifying political ideology for Iraq in the face of the evident failure or political bankruptcy of Baathist pan-Arabism. The book is richly illustrated with photographs of sculpture, monuments, paintings, and drawings reflecting the vacuity and totalitarian nature of Baathist "art" and the personality cult it has been designed to feed. Anyone acquainted with dissident Iraqi art, for instance the work of Jabr and other artists in exile in Italy, cannot fail to notice the lack of authenticity of official artistic production. Baram's writings on Iraqi politics, including his article "The Ruling Political Elite in Ba'ṭhi Iraq, 1968–1986; The Changing Features of a Collective Profile," a mine of information on the composition of the various organs of state (the Revolutionary Command Council, the cabinets, the National Assembly) over the period, are clearly the results of meticulous empirical research.²⁵ Occasionally, however, his attempts to draw wider conclusions from the information he presents do not sufficiently reflect the essentially arbitrary nature of present-day Iraqi politics.

The recent history of the Iraqi Kurds has not yet been written. The most succinct account is David McDowall's Minority Rights Group pamphlet *The Kurds*, updated in September 1991; the most detailed and comprehensive study of the period until 1975 is Chris Kutschéra's *Le Mouvement nationale kurde* (1979).²⁶ There are a few useful articles on recent Shi'i politics: Hanna Batatu's "Iraq's Underground Shia Movements: Characteristics, Causes and Prospects," Chibli

²⁴ For Springborg's article, see *Middle East Journal*, 40 (1986): 33–52; for Marion Farouk-Sluglett's, see *Maghreb-Machrek*, 131 (January 1991): 3–12.

²⁵ Baram's article is in *International Journal of Middle East Studies*, 21 (1989): 447–93.

²⁶ Peter Sluglett has written a brief account of Iraqi Kurdish politics until 1985, "The Kurds," in CARDRI (Committee Against Repression and for Democratic Rights in Iraq), *Saddam's Iraq: Revolution or Reaction?* (London, 1986), 177–202.

Mallat's "Religious Militancy in Contemporary Iraq; Muhammad Baqer el-Sadr and the Sunni-Shia Paradigm," and Pierre Martin's "Les Chi'ites d'Iraq; Une Majorité dominée à la recherche de son destin."²⁷ Finally, there are two reliable accounts of the Iran-Iraq War: Shahram Chubin and Charles Tripp's *Iran and Iraq at War* (1988, 1991), which is particularly good on the internal effects of the war on politics and society in both states, and Dilip Hiro's *Longest War: The Iraq-Iran Military Conflict* (1989), which takes the story to the conclusion of hostilities. A refreshing feature of both books is that neither insists the conflict was a reflection of the "age-old hostility" between Arabs and Persians. Kamran Mofid's *Economic Consequences of the Gulf War* (1990) attempts to draw up a balance sheet for this fruitless and pointless conflict, estimating that the cost of the war substantially exceeded the entire amount earned by both Iran and Iraq since the two states began to sell oil on the world market in 1913 and 1931 respectively.

It is impossible to predict what effect recent events will have on Iraqi historiography. Clearly, as long as the present dictatorship lasts, Iraqis will continue to be unable to carry out serious research on their country's past. If the regime is removed and one of an altogether different type takes its place, many talented Iraqis, historians among them, will be able to return to their country and contribute to a variety of discussions and debates. Outsiders too will be able to have access to the kind of information and source materials that they have long wanted to consult. Iraqi history is a fascinating and fertile field for study, and those of us who work in it and are concerned for the country and its people look forward to the time when we can explore it in all its richness.

²⁷ For Batatu's article, see *Middle East Journal*, 35 (1981): 578–94; for Mallat's, see *Third World Quarterly*, 10 (1988): 699–729; Martin's essay is in *Peuples méditerranéens*, 40 (1987): 127–69.

Survey of Egyptian Works of History

AFAF LUTFI AL-SAYYID MARSOT

OVER THE PAST TWO DECADES, Egypt has seen a massive outpouring of works dealing with a variety of historical and political subjects. It would seem as though all the pent-up ideas censored by the Nasser regime poured out during subsequent years to quench the country's intellectual thirst. While this article will concentrate on historical works, we must perforce also examine works that pertain to disciplines necessary to the writing of history, such as religious discourse. The fundamentalist current that has swept the Muslim world has roused an interest in religious interpretations of history, economics, and gender relations.

According to Albert Hourani, the foremost historian today on the Arabs, "The writing of history was a feature of all urban Muslim societies . . . Works of history and cognate subjects provide the largest body of writing in the main languages of Islam, apart from religious literature."¹ That may be the reason for the large market in Egypt for works of history, for they recount a past that had been proscribed under Nasser. Historical works dealing with the century-and-a-half-long monarchy were simply not written. There is also a large market for works dealing with the more recent past that lay bare, or purport to lay bare, the inner workings of the Nasser and Sadat regimes.

As proof of this intellectual ferment, one has only to look at the list of books published by what in America would be described as a small private press, that of the renowned Madbuli, who brings out over a thousand titles each year. Although Madbuli is certainly literate and well read, rumors circulate that he is illiterate, perhaps because he prefers wearing a native jellaba to Western clothing and so projects an image of someone who is not westernized. Possibly, the rumor is a calumny spread by jealous competitors.² Madbuli started out as a bookseller of leftist works in a *bouquiniste* stall by the Ezbekieh gardens, then moved to a small shop, which grew, as did the demand for books, until his bookstore, now in Midan Talaat Harb, a centrally located square in the heart of modern Cairo, became the focus of anyone interested in acquiring important books on Egypt, and he became

I owe a debt of gratitude to my friend and colleague Dr. Ali Hilla Dessouki for pointing out some of the works mentioned and to Dr. Amira al-Azhari Sonbol for graciously supplying me with some of these works. I have not tried to give a list of books by Western scholars writing on Egypt, because these are more easily accessible; however, in the following footnotes, some of the seminal works are indicated.

¹ Albert Hourani, *A History of the Arab Peoples* (Cambridge, Mass., 1991), 200.

² Although all my books save one are written in English, Madbuli knew about them, showing that he was knowledgeable even in books written in foreign languages.

one of the most prestigious of publishers, renowned for his integrity, fairness to his authors, and his ability to detect works of scholarly value.

THE FIRST GENRE OF RECENT PUBLICATIONS consists of memoirs, biographies, and personal accounts. The most interesting works in this category are by former members of the old regime, or of the Revolutionary Command Council, the original thirteen officers who carried out the revolution of 1952, which ended the monarchy and began the republic under military oligarchs, Nasser and Sadat. Some of these works begin with the 1919 revolution or, better yet, present annotated memoirs of men involved in these events, for example, a two-volume work, *Mudhakkirat Saad Zaghlul* (The Memoirs of Saad Zaghlul), annotated by Abd al-Azim Ramadan.³

The 1919 revolution was the first grass-roots revolt in Egypt and, under the leadership of the Wafd party, headed by Saad Zaghlul, combined all social classes in a call for independence from British occupation. Egyptians view the 1919 revolution as the first step on the road to independence, and Zaghlul is thus enshrined in the pantheon of beloved patriots, somewhat similar to the position occupied in the United States by George Washington. Zaghlul's memoirs, recorded throughout his lifetime, were deliberately written in a disguised script that is very difficult to decipher, even using a magnifying glass, for he did not want anyone to read them. Some of what he wrote could be regarded as treachery toward his colleagues—he mentioned plotting to get rid of them before they got rid of him—some as reflecting an overweening ego, and some as pure malice toward his associates and fellow nationalists. Most important to Egyptians, the memoirs recount Zaghlul's version of critical years for Egypt, 1914 to 1927, and the backstage negotiations that went on between the Egyptian politicians and the British, who, despite their declaration of Egyptian independence in 1922, were only willing to grant a token independence as a sop to nationalist agitation for self-government. They did not contemplate relinquishing their stranglehold on the Suez Canal, the lifeline of the British empire, or their naval base in Alexandria that allowed control of the Mediterranean, at least not until they lost their empire after World War II. These memoirs are indispensable to any historian of the period around 1919 and up to Zaghlul's death in 1927. They reveal the monumental ego of a famous man who had feet of clay, as many powerful men do, and who, even though he was a thorough nationalist, could not help but believe that in aggrandizing himself he was also helping his nation—not necessarily the case. These are the memoirs of a flawed giant, frustrated unfairly in his ambitions by the British authorities. Zaghlul could have done great things for

³ Abd al-Azim Ramadan, ed., *Mudhakkirat Saad Zaghlul*, 2 vols. (Cairo, 1987–88); one of the best works on the modern history of Egypt is Jacques Berque's monumental work, *L'Égypte: Impérialisme et révolution* (Paris, 1967), translated into English by Jean Stewart, *Egypt: Imperialism and Revolution* (London, 1972); see also Afaf Lutfi al-Sayyid Marsot, *Egypt's Liberal Experiment, 1922–1936* (Berkeley, Calif., 1977). One of the soundest works on an earlier period, that of the Urabi revolution of 1881 and the British occupation the following year, was written by Alexander Schoelch, *Ägypten den Ägyptern! Die politische und gesellschaftliche Krise der Jahre 1878–1882 in Ägypten* (Freiburg, 1972), translated as *Egypt for the Egyptians* (London, 1981).

Egypt had he not been ousted from the premiership when the British wrongly blamed him for inciting the assassination of Lee Stack, the commander-in-chief of the army, in November 1924.⁴

The memoirs of another nationalist, Abd al-Rahman Fahmi, edited by Yunan Labib Rizq, enrich our knowledge of that same period by giving an account from a different political point of view.⁵ Fahmi led the Wafd party when its leaders, Zaghlul and others, were in exile, and he created a secret department designed to carry out political assassinations. On Zaghlul's return from exile, Fahmi was replaced, and he nursed a grudge against Zaghlul. Fahmi recounts how he set up the assassination department and how he was the brains and organizer behind the nationalist movement, not Zaghlul.

The memoirs of Hasan Yusif, the grand chamberlain under the monarchy, the brief reigns of Fuad and his son Faruk, 1922–1952, treat events of a later period from the viewpoint of one who was closely associated with the palace and privy to many of the intrigues in the higher echelons of power.⁶ It is a discreet book, however, typical of a man who dealt diplomatically with opposing factions and was forced to set his personal feelings aside. Some might consider it a whitewash of the monarchy, especially under Faruk, since it was written during the Sadat period, when it once again became possible to write about the monarchy in other than purely negative terms. While many have accused Faruk of conniving to enrich himself by buying defective arms, Yusif claims that he personally saw no proof for such allegations and tries to diminish the extent of corruption attributed to the king and his friends.

Memoirs of individuals involved in politics but not holding political office also cast light on the events of the century. Among these works are the memoirs of Louis Awad, *Awraq al-Umr* (The Papers of a Lifetime). He was a leftist professor of literature who became a prominent journalist and a popularizer of history, political theory, and current events. Awad's work is important because he was involved with the Communist party before the 1952 revolution and was in opposition during the Nasser and Sadat years; thus his political knowledge encompasses both prerevolutionary and postrevolutionary leaders. His autobiography also provides a fascinating account of the family background and education of a young Coptic Egyptian in the city of Asiut early in the century.⁷

These are a few examples of the kind of memoirs now available in print. They contain invaluable material for social as well as political historians and present controversial, conflicting, sometimes libelous claims regarding critical periods of Egyptian history, especially those from 1919 onward. The officers who led the 1952 revolution claimed that they were concluding the Zaghlulist revolution, but they tried to discredit all the past political history of Egypt and its leaders in order to appear as the country's only true patriots. Therefore the generation that grew up after 1952 received a biased account of past regimes and their leaders, especially of the Wafd party, the only secular grass-roots organization, which the

⁴ From then on, the British blocked Zaghlul's path to the premiership even when his party won the majority vote.

⁵ Yunan Labib Rizq, *Mudhakkirat Abd al-Rahman Fahmi: Yawmiyat Misr al-siyasiyah* (Cairo, 1988).

⁶ Hasan Yusif, *al-Qasr wa dawruh fi-l Siyasa al-Misriyya* (Cairo, 1982).

⁷ Louis Awad, *Awraq al-Umr: Sanawat al-takwin* (Cairo, 1989).

military accused of collaborating with the British occupiers rather than leading a genuine movement for independence.

Memoirs written by men of the Revolutionary Command Council begin with Nasser's so-called autobiography, ghost-written by Tom Little, a British journalist, probably based on talks with Nasser in the early days of the regime. In this work, Nasser reveals that his hatred for the elite stemmed from a slight he felt when a cabinet minister who made it possible for him to enter the military academy asked him to sit near the chauffeur. The book lays out the three circles of power that Nasser believed he should lead: the African, the Arab, and the Muslim. Anwar Sadat produced two versions of his biography, which contradict each other in some details, the later version making him appear in a better light. The first one, *Unknown Pages*, was withdrawn when he became president, while the second version, *In Search of Identity*, gives him a more heroic role and claims that he was negotiating a treaty with the Germans that would free Egypt during World War II.⁸

Abd al-Latif al-Baghdadi, a former minister of public works, serialized his memoirs in one of the popular weeklies in 1977. He was one of those who broke with Nasser, and the parting of ways left him a bitter opponent of the Nasser regime and of the Sadat regime also, but he is well respected for his principled stand in choosing resignation over clinging to office, a rare step. Baghdadi accuses the cabinet of setting up a dictatorship and condoning corruption. Tharwat Ukasha, the former minister of culture, has recently brought out his memoirs, in two volumes. They are among the most fascinating of the accounts by the military revolutionaries, for he was probably more interested in art and culture than in politics and power and so provides a review of the artistic life of Egypt in the 1950s and 1960s. Both Muhammad Naguib, in his book, *Kalimati li-l tarih* (My Word for History), and Kamal al-Din Ibrahim, who also resigned from his cabinet post when he disagreed with Nasser, give accounts contradictory to those by Nasser concerning who started the revolution, who wrote the early declarations, and who did what to whom.⁹ Ibrahim, for example, accuses Nasser's hangers-on of having killed Abd al-Hakim Amir and then announcing that Amir had committed suicide. Ibrahim's long newspaper interview and those of various other revolutionary members were solicited by Mahmud Fawzi in a series titled "The July Revolutionaries Speak," first published in the weekly *October* in the summer of 1987. An earlier series by Salah Muntasir, a famous journalist, ran in the daily *Al-Ahram* on July 24, 1983. These works carry more important information than the official archives, for the archives have been purged of controversial or embarrassing records, while the memoirs relate events as the authors

⁸ Gamal Abdel Nasser, *The Philosophy of the Revolution* (Cairo, 1954); Anwar al-Sadat, *In Search of Identity: An Autobiography* (London, 1978); Muhammad H. Haykal compares the two versions in his book, *Autumn of Fury: The Assassination of Sadat* (London, 1983). A number of works have been written on Nasser, such as R. Hrair Dekmejian, *Egypt under Nasser: A Study in Political Dynamics* (Albany, N.Y., 1971); a recent work on Sadat is Raymond A. Hinnebusch, Jr., *Egyptian Politics under Sadat: The Post-Populist Development of an Authoritarian-Modernizing State* (Cambridge, 1985). An insightful and sober comparison of the two men and their regimes is given in Mark N. Cooper, *The Transformation of Egypt* (Baltimore, Md., 1982); and John Waterbury, *The Egypt of Nasser and Sadat: The Political Economy of Two Regimes* (Princeton, N.J., 1983).

⁹ Muhammad Naguib, *Kalimati li-l tarih* (Cairo, 1986).

remember them. One account must be balanced against another to get at the real story of how the revolution of 1952 was planned and executed, who the important personalities were, what happened both internally and externally to derail the revolution and allow an oligarchy to seize power and rule absolutely. The definitive account of the 1952 revolution remains to be written, for no one has yet collated and analyzed the many accounts.

THE SECOND GENRE OF PUBLICATIONS encompasses scholarly monographs written by trained historians who have done yeoman work in the archives. Perhaps an element of nationalism has influenced these scholars, for the history of Egypt had, until the 1950s, been written from a Eurocentric point of view, with little attention paid to Egyptian sources. The 1950s and 1960s were a period of decolonization and nationalist pride; Egypt had finally gained independence from British occupation in 1954, and, as a consequence, an awareness of self grew among historians. They felt that their past had been treated unfairly and viewed in a biased fashion through a colonial lens. Setting out to right the balance by using local sources, Egyptian revisionist historians found them to be just as useful and insightful, presenting as they did an indigenous frame of reference. Some of the most worthwhile publications deal with the eighteenth century, the age of the mamluks and Ottoman Egypt.¹⁰

Questions of peasants and land tenure have much interested historians, for the peasant was exploited unmercifully by all Egyptian rulers until Nasser passed the laws limiting land tenure to fifty acres per person. Who owned land, why, and how are burning historical issues in a country where the majority of the population are of peasant origin. The role played by Egyptians during the Ottoman age is also regarded as important, for the Ottoman army of occupation gradually became staffed by Egyptians. The native elites, the merchants, and the men of learning, especially religious learning (the ulama), served as the bridge between the alien slave rulers, the mamluks, and the ruled; hence the rising interest in how Egypt was administered and by whom. Finally, historians have asked if Egypt was ruled differently from the rest of the Arab territories, and if so, why? Was Egypt part and parcel of these territories, a central core of the Arab world, even under Ottoman domination? Many of these historical themes have been of immediate

¹⁰ The mamluks were a military oligarchy of ex-slaves that ruled over Egypt from 1250 until the Ottoman conquest in 1516. Because the Turks allowed them to acquire administrative positions, they were able to regain control of Egypt in the eighteenth century. Their rule was terminated by Muhammad Ali (1805–1849).

Egyptians are not the only ones to bring to light new archival material, for André Raymond, with his masterly *Artisans et commerçants au Caire au 18^{ème} siècle*, 2 vols. (Damascus, 1973–74), has introduced many of us to the *waqf* documents and the archives listing property of deceased people, on which a tax was levied by the mamluks and the *ocaqs* (regiments). Stanford J. Shaw has given us *Ottoman Egypt in the Age of the French Revolution* (Cambridge, Mass., 1964); and *The Financial and Administrative Organization and Development of Ottoman Egypt, 1517–1798* (1958; Princeton, N.J., 1962); Daniel Crecelius has also worked extensively on the eighteenth century and its archives, see *The Roots of Modern Egypt: A Study of the Regimes of Ali Bey al-Kabir and Muhammad Bey Abu al-Dhahab, 1760–1775* (Chicago, 1982). Together, Crecelius and Abd al-Wahhab Bakr have translated and annotated the Damurdashi work, *Al-Damurdashi's Chronicle of Egypt, 1688–1755* (Leiden, 1991); Peter Gran, *Islamic Roots of Capitalism: Egypt, 1760–1840* (Austin, Tex., 1979).

political concern, as Egypt has claimed first the leadership of the Arab world, then that of the Muslim world. Other issues have arisen from the influence of the Annales school or in the desire to write history from the point of view of the masses, not the elites. Thus issues of power, of resistance to power, and the means used for such resistance, gender relations, and the role of women in Muslim societies became important. Social and economic history rather than military or diplomatic history have claimed the attention of historians.

It is interesting to note that the Nasser regime discouraged study of any period of the Muhammad Ali dynasty, that is, the nineteenth and twentieth centuries, unless it was evaluated negatively. The regime sought to undermine the achievements of the monarchy in order to justify the decision to put an end to it, claiming it was a corrupt institution that had done nothing but harm to the country. For their part, some historians saw a parallel between the age of the mamluks in the eighteenth century and the Nasser regime: both periods brought military oligarchs to power as heads of state. Implicitly, some used the history of the mamluks as a means of describing and opposing the regime, showing the negative aspects of military rule.

The Egyptian archives are an under-utilized treasure trove in part because of the limited facilities available to the researcher in terms of tables, chairs, and lighting, but also because of difficulties of access. In one archival depository, there are over 36,000 dossiers, only one of which has been indexed, and that one indexing took more than a year to carry out. Countless boxes contain loose pages of material, some of which has never been read by anyone other than the officials of the day, certainly not by scholars, as well as manuscripts going back to the Middle Ages and accounts of endowments (*awqaf*). Researchers who wish to work on the economic aspects of Egyptian history must learn a script used since the Middle Ages by the guild of bookkeepers, a kind of shorthand that made it impossible for someone outside the guild to understand their accounting. On the other hand, those wishing to read documents of endowments dating from the eighteenth century or earlier have to learn another script, *diwani*, used by the scribes, who also occasionally wrote in shorthand. These difficulties are somewhat akin to those encountered in deciphering medieval scripts in European archives. The only material indexed is that having to do with endowments or mortmain domains, and the index is structured according to the year in which the transaction was carried out. There is no alphabetical order. Thus, to discover the possessions of a certain individual, several years need to be scanned to find the first economic transaction and the last, plus all the intervening changes, since every piece of property bought, sold, or exchanged was recorded separately.

Among the invaluable works appearing recently is that by Muhammad Muhammad Amin, *Catalogue des Documents d'Archives du Caire*.¹¹ Introduced in French, the body of the work presents facsimiles of old Arabic documents and includes a critical edition of nine of them with a line-by-line rendition in modern

¹¹ Muhammad Muhammad Amin, *Fihrist wathaiq al-Qahirah hatta nihayat asr al-Salatin al-Mamalik (239–922H/853–1516M) maa nashr wa-tahqiq tisat namadhij* [*Catalogue des Documents d'Archives du Caire*] (Cairo, 1981).

Arabic script. The book is of use to historians not only because of the documents edited but also because comparison of the photographs of the documents and the rendition of their content allows one to learn how to read that script. To date, there is no other way to learn the script save by trial and error.

For the eighteenth century, we have the works of Abd al-Rahim, who brought out the first substantial account of the peasants of Egypt, *Al-Rif al-Misri fi-l qarn al-thamin ash'r* (The Egyptian Countryside in the Eighteenth Century), a socio-economic study based on economic and financial records he himself discovered in the archives, the *tarabi* or tax records. A few years later, a complementary study by Ali Barakat appeared, *Tatawwur al-Mulkiya al-Ziraiya fi Misr: 1813–1914* (The Development of Agricultural Landownership in Egypt: 1813–1914). Laila Abd al-Latif Ahmad fleshes out the period of Ottoman rule in Egypt with her history of its government and administration of Egypt in two works, *Al-idara fi Misr fi-l asr al-Uthmani* (Administration in Egypt in the Ottoman Age) and *Dirasat fi tarikh wa muarikhhi Misr wa-l Sham* (Studies in the History and the Historians of Egypt and Syria in the Ottoman Age). Abd al-Wahhab Bakr treats the same era and includes accounts of women. Both Bakr and Abd al-Rahim have brought out annotated and edited volumes dealing with manuscript sources from the eighteenth century.¹² The archival material used in all these accounts makes a major contribution to the history of unknown and understudied periods, and the authors' interpretations, based on that material, are new and exciting, for they clearly show a historical continuity from the ascendancy of the beylicate, beginning in 1711, well into the nineteenth century.¹³ They also demonstrate that the era was not dominated by pointless turmoil so much as by struggles over economic issues exacerbated by the penetration of the Western market system into the region. Peasants were turning to cash crops instead of a subsistence economy and were being subsidized by merchants who sought to export these crops. While in theory the elites controlled the land, in practice peasants had certain traditional rights also, such as the right to farm the land so long as they paid taxes on it. Even more important, these histories clearly show that land was treated as private property in the eighteenth century long before the land law of the mid-nineteenth century and was controlled by women, merchants, and the ulama, as well as by the elites.

Quarrels and battles between mamluks were motivated by economic needs, for they lost their monopoly over the land and had to find other means to support themselves and their retainers. The advent of the Western market system

¹² A. A. Abd al-Rahim, *Al-Rif al-Misri fi-l qarn al-thamin ash'r* (Cairo, 1974); Ali Barakat, *Tatawwur al-Mulkiya al-Ziraiya fi Misr: 1813–1914* (Cairo, 1977); Laila Abd al-Latif Ahmad, *Al-idara fi Misr fi-l asr al-Uthmani* (Cairo, 1978); *Dirasat fi tarikh wa muarikhhi Misr wa-l Sham* (Cairo, 1979). Abd al-Wahhab Bakr, "Al-Ilaqat al-ijtimaiya fi Misr al-Uthmaniya," in Abd el Jelil Temimi, ed., *Al-Hayat al-Ijtimaiya fi-l wilayat al-Arabiya athna al-ahd al-Uthmani* (Zaghouan, 1988). See also articles by Abd al-Rahim, K. Cuno, M. Deeb, A. el Dessouki on land tenure in Egypt in Tarif Khalidi, ed., *Land Tenure and Social Transformation in the Middle East* (Beirut, 1984).

¹³ Books written before the 1970s stressed the internecine warfare among the mamluks without giving any solid reason for the constant warfare, nor do they mention the changing economy, land tenure, or other issues of socioeconomic history. It is only the books published recently that have given us sound economic reasons for mamluk violence. For the continuity of the beylicate into the nineteenth century, see my *Egypt in the Reign of Muhammad Ali* (Cambridge, 1984).

encouraged rulers to raise the price of raw materials and to export them, while reducing the price of finished goods locally in order to prevent raw materials from being used locally. A proletariat was thereby created out of artisans. To write an adequate history of that period, it is clearly necessary to refer to these works and to the archives they have brought to light. One hopes that similar accounts for the seventeenth century will be forthcoming, for much material on that period exists and is already being tackled by younger scholars, such as Nelly Hanna in *An Urban History of Bulaq in the Mamluk and Ottoman Periods*.¹⁴ I expect that new histories dealing with the nineteenth century will also be produced, since people are now free to reexamine that era, especially the earliest grass-roots revolution in Egypt, led by Ahmad Urabi, which resulted in the British occupation of 1882.¹⁵

A three-volume work described by the author, Muhammad Hasanain Haikal, as a journalistic account of history, brings to light interesting and novel documents derived from four countries. *Harb al-thalathin sanah* (The War of Thirty Years) chronicles the Suez War in 1956, the interwar period, and the June 1967 War. The last volume, titled *al-Infijar* (The Explosion) is over a thousand pages long and heavily documented.¹⁶ As Nasser's confidant, *éminence grise*, or *homme de main*, Haikal, officially chairman of the board of al-Ahram Enterprises, was privy to much information never made public and certainly not found in any archive. This information includes private conversations between heads of state, secret missions for Nasser, and details on incidents never noted in public reports. While some of the documents Haikal quotes, such as those from Israel and the United States, come from secondary works, the study remains a gold mine. The villain is Lyndon Johnson, who is portrayed as determined to cut Nasser down to size as a means of compensating for the disaster in Vietnam. Once again, it would seem that the Arabs were forced to pay the price for something that had little to do with them. Some people believe that the last bullet in the Vietnam War was fired in the Gulf. According to Haikal, Johnson conspired with Israel to punish Nasser, but, in order to make sure that the Israelis did not go too far, he sent the USS *Liberty* to spy on the Israelis. Israel was determined to extend its territory at the expense of the Jordanians, which was not part of the deal with Johnson. The Israelis tricked Jordan into entering the war by jumbling Egyptian-Jordanian codes, telling the Jordanians to attack when the Egyptian message was warning them to hold back because the Egyptian air force had been decimated. To cover their tracks, the Israelis bombarded the *Liberty*.¹⁷

THE ISRAELI VICTORY OVER EGYPT IN 1967 came as a shock to most Egyptians, since government propaganda had convinced them of the strength of the Egyptian army. Defeat prompted reflection and a search for new meaning and

¹⁴ Nelly Hanna, *An Urban History of Bulaq in the Mamluk and Ottoman Periods* (Cairo, 1983).

¹⁵ See Schoelch, *Agypten den Ägyptern*.

¹⁶ Muhammad H. Haykal, *1967 al-Infijar: Harb al-thalathin sanah* (Cairo, 1990).

¹⁷ For an account of the incident involving the *Liberty*, see James M. Ennes, Jr., *Assault on the Liberty* (New York, 1979), written by one of the crew.

national purpose. An important event was the alleged appearance of the Virgin hovering over a Coptic church in a poor suburb of Cairo, which both Muslims and Christians explained as a sign that the defeat resulted from God's displeasure with a people who had departed from righteousness. Military incompetence was not to be blamed.¹⁸ An awakening of religious consciousness followed, as did an increasing use of religious idiom in matters generally considered to be in the secular domain. Fundamentalism, or *intégrisme*, to use the more felicitous French term, resurged and directed historical works into several currents.

The first of these manifested a holistic approach to government and society, one that reflected the integrity of the whole of human experience. Its proponents believed that, in the past, Islam had been successful only when there was a symbiotic relationship between religion and society; without religion, there could be no morality, law, or even a valid political theory, for a state has to be built on an Islamic foundation. Among such works are those of two renowned and respected religious scholars, Shaykh Muhammad Abu Zahra, *Al-Mujtama al-insani fi dhill al-Islam* (Human Society in the Shadow of Islam), and Shaykh Muhammad al-Ghazali, whose *Al-Sunna al-Nabawiya* (The Prophetic Path), first published in January 1989, has been reprinted eight times. The writings of a Pakistani religious teacher, Abu-l Ala al-Mawdudi, who died in 1979, *Mafahim Islamiyya hawl al-din wa-l dawla* (Islamic Thought regarding Religion and the State) became equally popular because he too preached a religio-political integrity of thought, as did Sayyid Qutb, the ideologue of the Muslim Brothers, especially in his last work, *Maalim fi-l tariq* (Milestones). It has become the gospel of the more extreme *intégristes*, its message being that those who do not follow Islam faithfully are outside the pale and must be rejected. Hasan Hanafi, a leading Muslim Brother, brought out six volumes, *Fiqh al-turath* (The Jurisprudence of Our Cultural-Religious Legacy), that build on the notion of the closeness of culture, religion, and society.¹⁹ The terms *turath* (heritage) and *asala* (authenticity) became current in everyday talk with the accent on differentiating between cultural authenticity, which should be revived, and imported and synthetic accretions, which should be rejected.

The search for the authentic created an interest in the common people, for they were believed to be those who remained true to "traditional" ways of life, however one may define tradition. A populist current appeared and with it a focus on the "philosophy" of the common people: proverbs, folk tales, jokes. While the written

¹⁸ See the special issue of the *International Journal of Middle East Studies*, 12, no. 4 (1980), especially articles by Nazih Ayubi and Saad eddine Ibrahim, 423–53 and 481–99.

¹⁹ Muhammad Abu Zahra, *Al-Mujtama al-insani fi dhill al-Islam* (Cairo, n.d.); Muhammad al-Ghazali, *Al-Sunna al-Nabawiyah bayna ahl al-fiqh wa-ahl al-Hadiith* (Beirut, 1989); Abu al-Ala al-Mawdudi, *Mafahim Islamiyya hawl al-din wa-l dawlah* (Kuwait, 1974); Sayyid Qutb, *Maalim fi-l tariq* (Cairo, 1966); Hasan Hanafi, *Fiqh al-turath*, 6 vols. (Cairo, 1980). One of the best works on *intégrisme* is that by Gilles Kepel, *Le Prophète et le pharaon: Les Mouvements islamistes dans l'Égypte contemporaine* (Paris, 1984), translated as *The Prophet and the Pharaoh*; also see Hamid Inayat, *Modern Islamic Political Thought* (Austin, Tex., 1982); and Fazlur Rahman, *Islam and Modernity* (Chicago, 1982); a most interesting book is Roy Mottahedeh, *The Mantle of the Prophet: Religion and Politics in Iran* (London, 1985). In French, two excellent works are Bruno Etienne, *L'Islamisme radical* (Paris, 1987); and Jacques Berque, *L'Islam au défi* (Paris, 1980). See also Barbara Freyer Stowasser, ed., *The Islamic Impulse* (London, 1987); and John Esposito, *Islam and Politics* (Syracuse, N.Y., 1984).

word belongs to the literate, the proverb is believed to be a creation of the common people and summarizes pithily a world view, social relations, gender relations, and all aspects of daily life. Thus several works on proverbs have appeared, among them *Ala ray al-mathal* (As the Saying Goes) by Sulaiman A. Sulaiman and *Al-Amthal al-Shaabiyya* (Popular Proverbs) by M. Qandil al-Baqli.²⁰ Some of these works attempt to analyze the content of the proverb and to place it in its socio-historical context.

The second current deals with gender relations, women, and their involvement in public life. A battle royal broke out between the *intégristes*, some of whom tried to limit women's role to that of childbearing and rearing and toward that end brought out volumes reinterpreting religious rulings to show that women should be secluded. The secularists and other *intégristes* also appealed to the past and reinterpreted it to show that Islam had encouraged women to become involved in public life.

Zainab al-Ghazali, a leader of the Muslim Women's Association, an offshoot of the Muslim Brothers, wrote an autobiography in which she preached a limited role for women and laid down their dress code. By then, the question of Islamic garb had become hotly debated. The more radical female *intégristes* wore masks, *niqab*, while the rest wore various combinations of modest attire. Others, like a highly respected scholar who writes under the nom de plume of Bint al-Shati, interpreted the past to show that the Prophet had liberated and respected women. Shaykh al-Ghazali, who had been reactionary in his views regarding women, tries in his 1990 work, *Qadaya al-Mara bain al-taqalid al-rakida wa-l wafida* (The Legal Condition of Woman between Stagnant and Recent Traditions), to redress the balance in the religious role ascribed to women. He points out that the Quran specifies the right of women to work and their right to equality of treatment with men. He argues that neither they nor their voices are *awra* (weak spot, blemish, imperfection), as some *intégriste* group members claimed. (In Pakistan, the sign on the women's public facilities reads "Awra"). An even more liberal interpretation of the role of women is given in Hashim Sharif's work *Al-Mara al-Muslima* (The Muslim Woman). He traces the "myths" that denigrate women in Islamic literature to political circumstances and personal animosities among some authors and Aisha, the Prophet's wife. A Moroccan scholar, Fatima Mernissi, carries out a similar exegesis in her recent work on the Prophet Muhammad and his wives.²¹

The interest in women's roles in society was generated by a number of causes: economic, for it was cheaper for women to wear so-called Islamic garb than to follow the latest fashions, especially during a time of inflation; political, as a statement of a return to one's roots and a negation of westernization and

²⁰ Sulaiman A. Sulaiman, *Ala ray al-mathal* (Cairo, 1986); Muhammad Qandil al-Baqli, *Al-Amthal al-Shaabiyya* (Cairo, 1987).

²¹ Zaynab al-Ghazzali, *Ayyam min Hayati* (Cairo, 1978); Bint al-Shati, *Al-Quran wa qadaya al-insan* (Beirut, 1975); M. al-Ghazali, *Qadaya al-Mara bain al-taqalid al-rakida wal-l wafida* (Cairo, 1990); Hashim Sharif, *Al Mara al-Muslima* (Cairo, 1982); Fatima Mernissi, *Le Harem politique: Le Prophète et les femmes* (Paris, 1987). There is also a recently published bibliography of works on women in the Arab world by Margot Badran and Miriam Cooke, *Opening the Gates: A Century of Arab Feminist Writing* (London, 1990).

consumerism that the country could not afford; social, as a consequence of more women seeking jobs in the marketplace and competing with men for them.²²

Another outgrowth of *intégrisme* has been a number of works dealing with civil and political rights of citizens. While such subjects normally belong to the realm of secular politics, they were given a religious coloration when authors implied that they were writing history to disclose wrongs and that good Muslims are bound by conscience to reveal injustice and attack it. One of the most critical studies of the Nasser and Sadat regimes is by Husain Munis, *Bashawat wa-subar bashawat* (Pashas and Super Pashas). In a devastating indictment, he compares these revolutionary governments with the old regime—the pashas—and defines the hangers-on around Nasser and Sadat as “super pashas.” These military men who led the intelligence network and served as the country’s ministers prevented Nasser and Sadat from discovering the truth of what was happening in the state. They abused their power and were more corrupt, greedy, and damaging to the nation than the pashas had ever been. Munis compares these super pashas to the mamluks of past centuries. They lied and cheated their way to riches but were never punished because they were the ruler’s supporters. He accuses the super pashas of having robbed Egypt and stolen the wealth of the royal family, much of which was sold abroad, and gives shocking examples of such abuse. He charges that “both regimes, the Nasserist and the Sadatist, failed because they lied to the people, for everything appeared as though it were a play, with bad actors, but still actors.” Clearly, his message is that abuse of power occurs in absolutist regimes because there is no accountability to a public; loyalty to the ruler displaces all other virtues and leads people to depart from religious norms of good conduct.²³

By contrast, *intégrisme* among the Muslims was met by a rising tide of self-assertion from the Coptic minority in Egypt. After all, the apparition of the Virgin was seen hovering over a Coptic church. A new active Coptic pope, Anba Shenuda, had acceded to the see. He was an enlightened and educated man who wanted to revive a church that had fallen into the doldrums. Previous popes were managers, neither dynamic nor keen to galvanize the Coptic community into assertiveness out of fear of repercussions from the Muslim majority.

Anba Shenuda was charismatic, an engaging speaker who inspired the community. The political and economic conditions that had led Muslims to seek an alternative ideology and a return to religious idiom also led the Coptic community to do the same. Just as much Muslim-oriented literature has appeared, a massive quantity of Coptic literature has also been published, including Anba Shenuda’s biography, accounts of his exile by Sadat (who accused him of fomenting discord between the two communities), and histories of the church and its revitalization. Among the many works on the Coptic minority are Mahmud Fawzi’s *Al-Baba*

²² See the article by John Alden Williams, “Veiling in Egypt as a Political and Social Phenomenon,” in John L. Esposito, ed., *Islam and Development: Religion and Sociopolitical Change* (Syracuse, N.Y., 1980); see also Nazih Ayubi, “Secularism and Modernization,” *Free Inquiry*, 2 (Winter 1981): 18 and following; Earle L. Sullivan, *Women in Egyptian Public Life* (Syracuse, N.Y., 1986); A. al-Sayyid Marsot, “Women and Social Change,” in Georges Sabagh, ed., *The Modern Economic and Social History of the Middle East in Its World Context* (Cambridge, 1989).

²³ Husain Munis, *Bashawat wa-subar-bashawat: Surat Misr fi asrayn* (Cairo, 1988).

Shenuda wa tarikh al-kanisa al-Kiptiyya (Anba Shenuda and the History of the Coptic Church), 1991; Rafiq Habib's *Al-Masihyya al-siyasiyya fi Misr* (Political Christianity in Egypt), 1990; Habib's *Ihtijaj al-dini wa-l sira al-tabaqi fi Misr* (Religious Protest and Class Conflict in Egypt), 1990; Musad Sadiq's *Dhahirat tajali Um al-Nur* (The Phenomenon of the Manifestation of the Mother of Light), 1991, which discussed the miracle of the Virgin's apparition and the medical cures that it had effected. The rest of the works discuss the history of the Coptic church and the role played by Anba Shenuda in revitalizing it. Some claim to see a class struggle behind religious conflict, while others see political motives.

The last in the historical currents generated by the search for an ideology is a major debate on who should own the loyalty of the common people: the state, Arabism, or religion? For many decades, Arabism has been the dominant ideology among the Arab peoples. Arabism asserted Arab sovereignty over state sovereignty but was never given more than lip service by most governments, even if it may have remained attractive to many, if not most, Arabs. Deep down, there may have been a feeling that sovereign states, at least among the countries of the Asian portion of the Arab world (the Mashriq) were a Western imposition that created frontiers with little foundation in reality. Egypt, perhaps the only Arab country with recognizable borders that had existed for millennia, acted as a conduit for ideas between the Mashriq and the Maghrib. By making a separate peace with Israel through the Camp David Accords, Egypt encouraged a current of national particularity, and the Arab world was torn between an Arab ideology and parochial sovereignty. Pan-Islam, an alternative ideology that runs counter to Arabism, favors a union of all the world's Muslim nations. All three currents are alive and well in the Arab world, and all three contradict one another. The Gulf crisis and war brought these contradictions to the surface, but intellectuals have been writing about them for at least two decades without being able to resolve them. Of particular interest are Muhammad Abid al-Jabri's work, *Ishkaliyat al-fikr al-Arabi al-Muasir* (The Ambiguities of Contemporary Arabic Discourse) and Hisham Sharabi's *Neopatriarchy: A Theory of Distorted Change in Arab Society*.²⁴ These books discuss the struggle intellectuals are conducting to decide which of the three currents should predominate. They argue that the lack of clear direction in Arabic thought is the outcome of such currents competing for supremacy. Leftists and secularists seem to be in favor of Arab nationalism, religious people favor the Islamic current, minorities favor state sovereignty, and the rest of the population is bewildered. Until these issues are resolved, the soul of a people is up for grabs, and citizens continue to be bombarded by claims in favor of each ideology.

A fourth type of recent publication treats the ethos of a nation, the characteristics that distinguish one people from others, such as Jamal Hamdan's monumental work, *Shakhsiyat Misr* (The Personality of Egypt).²⁵ Hamdan explores the

²⁴ Muhammad Abid al-Jabri, *Ishkaliyat al-fikr al-Arabi al-Muasir* [The Ambiguities of Contemporary Arabic Discourse] (Beirut, 1985); Hisham Sharabi, *Neopatriarchy: A Theory of Distorted Change in Arab Society* (London, 1988).

²⁵ Jamal al-Din Mahmud Hamdan, *Shakhsiyat Misr: Dirasah fi abqariyat al-makan*, 4 vols. (Cairo, 1981–84).

elements that he considers unique to Egyptians and that have helped form their picture of geography, human nature, and the economy.

Finally, it is worth noting that nearly half the population of Egypt is illiterate and cannot avail itself of the sources of information discussed here. They must resort instead to radio, television, theater, and films. The ideas spread by these media deserve a study to themselves, for, while radio and television are still under government control, the rise in satire and symbolism in plays and movies presenting historical and political events and the different roles proposed for women (ranging from total liberation to religious seclusion) are basic to the creation of the new Egyptian ethos.

The Arabian Peninsula in Modern Times: A Historiographical Survey

J. E. PETERSON

IN 1935, THE BRITISH FOREIGN OFFICE produced a memorandum called "The Seven Independent Arabian States."¹ The independent states in the Arabian Peninsula still number seven. But of those described a half-century ago (Yemen, 'Asir, al-Hijaz, Najd, Kuwait, Jabal Shammar, and Jawf), only two still exist in similar form. Today, six of the seven states are monarchies: Saudi Arabia, Kuwait, Bahrain, Qatar, the United Arab Emirates (UAE), and Oman. The seventh, the Republic of Yemen, was formed in 1990 out of the merger of North and South Yemen.

This immense change provides an illustration of the fragile and transitory nature of traditional Arabian states, given their foundations on shifting tribal allegiances, their absolute dependence on strong and capable leadership, and their lack of firm territorial grounding. But today's seven states have a more durable appearance, even able to withstand the recent conquest of one by a rapacious neighbor. Obviously, much has transpired in just a half-century to change the situation. It can be argued that the process of state formation is the single most fundamental factor in the modern history of the Arabian Peninsula. Earlier writing on the Peninsula dealt mostly with rulers and tribes. More recent writing, whether scholarly or otherwise, has concentrated heavily on country case studies. Much of this is political history ("names and dates"), but the country emphasis also holds true generally for what little economic and social history exists. As such, state formation provides a convenient and useful prism through which to view the historiography of Arabia.

At first glance, the literature on Arabia seems misleadingly substantial. The majority of it is descriptive or narrative. Until the last several decades, most authors have been either travelers, both casual and "professional," or government officials (principally British) stationed in the region. It should not be surprising

¹ United Kingdom, India Office Library and Records, L/P&S/18/B446, "The Seven Independent Arabian States," W. J. Childs, Foreign Office, May 1935. For the sake of clarity and brevity, I have adhered in this essay to the following conventions: use of "the Gulf" refers to the Arabian or Persian Gulf; "Gulf states" means the six member-states of the Gulf Cooperation Council; "the region" means the Arabian Peninsula, as do the terms "Arabia" and "Arabian"; transliteration of Arabic words and names conforms generally with the Library of Congress system except that diacritical marks have been omitted, apart from the 'ayn and hamza; references are almost exclusively to books, except where a seminal article may be concerned with a subject not otherwise treated (this should not be interpreted as an implicit judgment against the value of periodical literature). Most references are published in English, although I have cited a few indispensable Arabic works.

that the travel literature has been of mixed quality. Nevertheless, the accounts of intrepid travelers such as Carsten Niebuhr, George Sadleir, J. R. Wellsted, Charles Doughty, Theodore Bent and Mabel Bent, and Richard Burton often constitute the few surviving records of much of modern historical interest.² That tradition has been continued by such twentieth-century travelers, explorers, missionaries, and journalists as Bertram Thomas, H. St. John B. Philby, Paul Harrison, Claudie Fayein, Daniël van der Meulen, Wilfrid Thesiger, and David Holden.³ At the same time, diplomats and expatriate administrators have made seminal contributions to the history of Arabia, not unlike the way in which they collected specimens of geology or flora and fauna for analysis in museums back in Europe. Many of these works, among them Samuel Miles, Snouck Hurgronje, and Arnold Wilson, provide the base on which later writers, both Western and indigenous, have been able to build.⁴ As a source on subjects as diverse as political narratives, economy, slavery, telegraphs, and tribal gazetteers, J. G. Lorimer's compilation is without peer.⁵

While the factual outlines of the development of the seven states are fairly well known, little work has been done to fill in the contours. All too often, new writing consists of a rehash of stories already told, frequently relying on the same secondary sources, or of superficial country surveys prompted by the region's high profile over the last decade or two. Scholarly attention to the Peninsula is largely the product of the last quarter-century. It is relatively sparse and variable in its quality. With few exceptions, historical scholarship has yet to move beyond the comfortable horizons of country studies and political analysis. One feature of Saudi Arabia's socioeconomic development and the emphasis placed on higher education has been the creation of a body of scholarly work, generally in the form of unpublished doctoral dissertations, by Saudi students, many of whom later rose to high positions in the government. One wonders to what extent these students were able to translate their ideas into practice upon their return home to government service.

Broadly speaking, two sorts of state formation in the Arabian Peninsula can be distinguished: the longstanding proto-states of Yemen and Oman, based in part

² Carsten Niebuhr, *Travels through Arabia, and other Countries in the East* (Edinburgh, 1792); George Forster Sadleir, *Diary of a Journey across Arabia (1819)* (1866; rpt. edn., Cambridge, 1977); J. R. Wellsted, "Narrative of a Journey into the Interior of Oman," *Journal of the Royal Geographical Society* (1837): 102–13; Charles M. Doughty, *Travels in Arabia Deserta* (London, 1926); J. Theodore Bent and Mabel V. A. Bent, *Southern Arabia* (London, 1900); Richard F. Burton, *Personal Narrative of a Pilgrimage to Al Medinah and Meccah* (London, 1907).

³ Bertram Thomas, *Alarms and Excursions in Arabia* (London, 1931); and *Arabia Felix: Across the Empty Quarter of Arabia* (London, 1932); Paul W. Harrison, *Doctor in Arabia* (New York, 1940); Claudie Fayein, *A French Doctor in the Yemen* (London, 1957); Daniël van der Meulen, *Faces in Shem* (London, 1961); Wilfrid Thesiger, *Arabian Sands* (London, 1959); David Holden, *Farewell to Arabia* (London, 1966). Citations of Philby's works are given below, note 38.

⁴ S. B. Miles, *The Countries and Tribes of the Persian Gulf* (1919; 2d edn., London, 1966); C. Snouck Hurgronje, *Mekka in the Latter Part of the Nineteenth Century* (1931; rpt. edn., Leiden, 1970); H. R. P. Dickson, *The Arab of the Desert: A Glimpse into Badawin Life in Kuwait and Saudi Arabia* (London, 1949); and his *Kuwait and Her Neighbours* (London, 1956); Arnold T. Wilson, *The Persian Gulf: An Historical Sketch from the Earliest Times to the Beginning of the Twentieth Century* (1928; rpt. edn., London, 1959).

⁵ J. G. Lorimer, *Gazetteer of the Persian Gulf, Oman, and Central Arabia* (1908–15; rpt. edn., London, 1989). Other examples of the profitable partnership of policy and scholarship are United Kingdom, Admiralty War Staff, Intelligence Division, *A Handbook of Arabia* (London, 1916–17); and United Kingdom, Admiralty, Naval Intelligence Division, *Western Arabia and the Red Sea* (Oxford, 1946).

on their sectarian imamates, and tribal states elsewhere. There has been a fundamental revolution in the character of states in the Peninsula, a transformation from these traditional and tribe-dependent states to nation-states (or more accurately in some cases, city-states). It has not been a complete revolution, however. While in formal terms, the territorial states are modern creations, their roots are old, and the legitimacy of their ruling families is based on their evolution from traditional relationships. Modern structures have been superimposed on traditional methods. The way "things really work" is not all that different from traditional practice. These remain patrimonial societies and, with the exception of Yemen, retain patrimonial political systems.

Until comparatively recently, states in Arabia were minimalist, whether considered in terms of structure, functions, or their relationships with their citizens. In rural areas, the tribe was central to the individual's existence: in many ways, it formed something of a self-contained entity, politically, economically, and certainly socially. Allegiance to a larger state structure was ephemeral, produced either by force or transitory self-interest. Paul Dresch's study of tribes and politics focuses on Yemen, but it has considerable applicability elsewhere.⁶ While urban areas were largely outside the tribal sphere, they still formed a complex relationship with tribes. Tribes constituted the trading hinterland, controlled vital routes, provided the armed levies for rulers, and were a frequent threat to towns and cities. The impact of tribalism on state formation in the two traditional political entities of Yemen and Oman is as substantial as in those states evolving out of the leadership of a dominant tribal clan.

The panoply of Yemen's rich history has been profiled by Robert Stookey, as well as in a breezier account by Robin Bidwell.⁷ Yemen is now an isolated corner of the Middle East, but it is well known for its pre-Islamic civilizations. The subsequent long series of invasions and conquests serves as a microcosm for the currents of Islamic history. Traditional historians, such as 'Abd al-Wasi' al-Wasi'i and 'Abdullah al-Jarafi, focus on the dominant role of the Zaydi imamate over the last thousand years.⁸ This was a minimalist state whose imam was elected from the ranks of the great *sayyid* families (descendants of the Prophet Muhammad). The strength of the imams derived from the support of the northern tribes of the Zaydi sect, but the lack of an independent base of power was also their weakness. Strong imams extended their control from the northern highlands over the Sunni areas of the south and sought to keep the office within their families.

The nineteenth century saw the imamate in decline, threatened from the south by the British capture of Aden and from the north by a recrudescence of Ottoman interest. By the end of the century, however, an imam was elected from the Hamid al-Din, and his son Yahya (reign 1904–1948) took dramatic steps to strengthen and transform the imamate into a central government. Since many of the measures instituted by Imam Yahya were based on Ottoman practices (some of his officials were even former Ottoman officials who remained in Yemen

⁶ Paul Dresch, *Tribes, Government, and History in Yemen* (Oxford, 1989).

⁷ Robert W. Stookey, *Yemen: The Politics of the Yemen Arab Republic* (Boulder, Colo., 1978); Robin Bidwell, *The Two Yemens* (London, 1983).

⁸ 'Abd al-Wasi' Yahya al-Wasi'i, *Tarikh al-Yaman*, 2d edn. (Cairo, 1947); 'Abdullah 'Abd al-Karim al-Jarafi, *al-Muqtataf min Tarikh al-Yaman* (Cairo, 1951).

following the dissolution of their empire), it is unfortunate that there has been little work done on that formative influence.⁹

To European observers such as Hugh Scott, Harold Ingrams, and Basil Seager, the Yemen of the Hamid al-Din was a medieval anachronism.¹⁰ But, as Manfred W. Wenner shows, the process of neotraditional consolidation carried out by Yahya and his son Ahmad (reign 1948–1962) carried the seeds of its own destruction.¹¹ J. Leigh Douglas details the opposition movement that briefly took over in 1948 and provided inspiration for subsequent dissidents.¹² In September 1962, a coup d'état staged by the army plunged Yemen into five years of civil war, with Egypt supporting the nascent Yemen Arab Republic in the cities and the south and Saudi Arabia backing the royalist and Zaydi tribal forces in the extreme north. The war received international attention and involvement, as indicated by Dana Adams Schmidt's journalist's report and David Smiley's account of a British officer recruited to assist the royalists.¹³ National reconciliation was not fully achieved until 1970. A few years later, the army regained control of the state, which it has held ever since, despite assassinations and hostilities with South Yemen. Books by Mohammed Zabarah, Robert Burrowes, and myself detail the twin difficulties of building legitimacy around a narrowly based government while trying to carry out modest socioeconomic development with a minimum of resources.¹⁴

Meanwhile, in the southern half of Yemen, Aden had been taken over by the British in 1839 because of its strategic location, enhanced by the opening of the Suez Canal. As R. J. Gavin shows, for more than a century British policy vacillated between restricting its presence to Aden Colony itself and establishing a forward policy in the outlying protectorates.¹⁵ These consisted of a myriad of petty states with traditional rulers mired in stagnation compared with urbanized and bustling Aden. A border agreement with Istanbul demarcated the northward limits of British influence, but it was never recognized by the Zaydi imams or subsequent republic, resulting in constant tension along the border. To the east, the Hadramawt and Mahra country also fell under British sway. Even more isolated, these areas received scant political or economic attention until after World War II, as Harold Ingrams, the first resident political officer, describes.¹⁶

⁹ For an exception, see Jon E. Mandaville, "Memduh Pasha and Aziz Bey: Ottoman Experience in Yemen," in B. R. Pridham, ed., *Contemporary Yemen: Politics and Historical Background* (London, 1984), 20–33. Also relevant in this regard is William Ochsenwald, *Religion, Society, and the State in Arabia: The Hijaz under Ottoman Control, 1840–1908* (Columbus, Ohio, 1984).

¹⁰ Hugh Scott, *In the High Yemen* (London, 1942); W. Harold Ingrams, *The Yemen: Imams, Rulers, and Revolutions* (London, 1963); Basil W. Seager, "The Yemen," *Journal of the Royal Central Asian Society*, 42 (July–October 1955): 214–30.

¹¹ Manfred W. Wenner, *Modern Yemen, 1918–1966* (Baltimore, Md., 1967).

¹² J. Leigh Douglas, *The Free Yemeni Movement, 1935–1962*, Giovanni Chimienti, ed. (Beirut, 1987).

¹³ Dana Adams Schmidt, *Yemen: The Unknown War* (New York, 1968); David Smiley with Peter Kemp, *Arabian Assignment* (London, 1975). Smiley also describes the author's experiences in commanding the sultan of Oman's army during the 1950s rebellion in Oman.

¹⁴ Mohammed Ahmad Zabarah, *Yemen: Traditionalism vs. Modernity* (New York, 1982); J. E. Peterson, *Yemen: The Search for a Modern State* (London, 1982); Robert D. Burrowes, *The Yemen Arab Republic: The Politics of Development, 1962–1986* (Boulder, Colo., 1987).

¹⁵ R. J. Gavin, *Aden under British Rule, 1839–1967* (London, 1975).

¹⁶ Harold Ingrams, *Arabia and the Isles*, 3d edn. (London, 1966).

But the Aden Colony and Protectorates were also affected by the Arab cold war of the 1950s and 1960s. It was not long before political opposition to the British presence escalated into guerrilla warfare and acts of terrorism. The most reliable account of this episode is by Julian Paget, but one should also consult the personal recollections of various governors of Aden.¹⁷ The subsequent establishment of the only Marxist state in the Arab world and the frequent battles within its party for control have created a certain morbid interest by the outside world in the affairs of this small state, as reflected in the books by Joseph Kostiner and Helen Lackner.¹⁸ Western assertions that South Yemen was an aggressive state providing a platform for Soviet penetration of Arabia have been suitably punctured by Fred Halliday.¹⁹ His earlier *Arabia without Sultans*, based on firsthand visits to the Yemens, is a Marxist interpretation of political change in Arabia and external involvement.²⁰ Closed to the West, the changes and tensions within South Yemeni society largely remain a mystery, although Norman Cigar has provided insights.²¹ The two Yemen republics had ended several border wars with a reaffirmation of their longstanding commitment to unity, but their opposing political structures seemed to be an insurmountable barrier, as F. Gregory Gause III discloses in his history of the movement toward unification.²² Nevertheless, the two halves of Yemen did manage to unify in May 1990, under the leadership of the northern president.

On the opposite side of the Peninsula, Oman displays many similarities to Yemen, geographically, socially, and, until this century, politically.²³ The heartlands of both countries have long been dominated by tribes, banding together in great confederations under the leadership of especially dynamic shaykhs. The countries' isolation from the main centers of Islam and their mountainous terrain have allowed small Islamic sects to flourish, the Zaydis in Yemen and the Ibadis in Oman. The balance of power between the tribes has in both cases been exercised by elected imams, acting more as chairmen over tribal confederations than as rulers of central states. Strong imams tended to pass on office to their families, and so dynasties arose, declined, and were replaced by "pure" imamates. In the

¹⁷ Julian Paget, *Last Post: Aden 1964–67* (London, 1969). The recollections include Tom Hickinbotham, *Aden* (London, 1958); Charles H. Johnston, *The View from Steamer Point; Being an Account of Three Years in Aden* (London, 1964); and Kennedy Trevaskis, *Shades of Amber: A South Arabian Episode* (London, 1968).

¹⁸ Joseph Kostiner, *The Struggle for South Yemen* (London, 1984); and *South Yemen's Revolutionary Struggle, 1970–1985* (Boulder, Colo., 1990); Helen Lackner, *P.D.R. Yemen: Outpost of Socialist Development in Arabia* (London, 1985).

¹⁹ Fred Halliday, *Revolution and Foreign Policy: The Case of South Yemen 1967–1987* (Cambridge, 1990). For an overview of both Yemens' foreign relations, see Eric Macro, *Yemen and the Western World, since 1571* (London, 1968).

²⁰ Fred Halliday, *Arabia without Sultans* (1974; New York, 1975).

²¹ Norman Cigar, "State and Society in South Yemen," *Problems of Communism*, 34 (May–June 1985): 41–58; "Islam and the State in South Yemen: The Uneasy Coexistence," *Middle Eastern Studies*, 26 (1990): 185–203.

²² F. Gregory Gause III, "Yemeni Unity: Past and Future," *Middle East Journal*, 42 (Winter 1988): 32–47.

²³ See J. E. Peterson, "Legitimacy and Political Change in Yemen and Oman," *Orbis*, 27 (Winter 1984): 971–98.

case of Oman, at least, this development has been seen to be so regular as almost to take a cyclical form, as postulated by J. C. Wilkinson.²⁴

With the election of an Al Bu Sa'id as imam in the mid-eighteenth century, the last cycle was interrupted, perhaps permanently. Robert G. Landen sees the cause of this development as modernization.²⁵ But the ruling Al Bu Sa'id family, having moved from the interior to Muscat on the coast and given up the title of imam, was weakened also by the division of the state into its Zanzibari and Omani components in the mid-nineteenth century.²⁶ Briton Cooper Busch shows how, at the same time, increasing British concern with European rivals in the Gulf led to a formal position of superiority in Muscat, thus reducing Muscat to little more than a de facto protectorate.²⁷ Then followed a division of the country into the Al Bu Sa'id-controlled coast and an autonomous interior under a newly elected imam in, as Wilkinson points out, the realization of a renascent Ibadi movement.²⁸ This historical division has been reflected in the indigenous histories of modern Oman. Ibn Ruzayk reflects the Muscat or Al Bu Sa'id emphasis, while the Salimis (father and son) were activists in the nineteenth and twentieth-century imamate.²⁹

The split was not healed until the forces of the sultanate regained control of the interior in the 1950s. For the first time in nearly a century, an Al Bu Sa'id sultan, Sa'id ibn Taymur (reign 1932–1970), visited the heartland of his own country, making the epic overland journey from his southern province described by James Morris.³⁰ Still, the sultanate's problems were not over. Continuing dissatisfaction was prompted by Sa'id's parsimoniousness, petty restrictions, and not very efficient minimalist government.³¹ This was most pronounced in the southern province of Dhufar, which the sultan regarded as a personal estate. What began as nationalist rebellion acquired a Marxist character, relying on outside assistance, before extensive British help enabled the sultanate to put it down. The government's side of the war has been told by British soldiers, including Ranulph Fiennes, Tony Jeapes, and John Akehurst.³² Another vital factor in the subsidence of the rebellion was the replacement of the old sultan by his Sandhurst-educated son Qabus ibn Sa'id (reign 1970–). Since then, Oman has used its modest oil resources to develop extensively along the lines of the other Gulf monarchies.

²⁴ J. C. Wilkinson, *Water and Tribal Settlement in South-east Arabia: A Study of the Aflaj of Oman* (Oxford, 1977).

²⁵ Robert G. Landen, *Oman since 1856: Disruptive Modernization in a Traditional Arab Society* (Princeton, N.J., 1967).

²⁶ Patricia Risso, *Oman and Muscat: An Early Modern History* (New York, 1986).

²⁷ Briton Cooper Busch, *Britain and the Persian Gulf, 1894–1914* (Berkeley, Calif., 1967).

²⁸ John C. Wilkinson, *The Imamate Tradition of Oman* (Cambridge, 1987).

²⁹ G. P. Badger, ed. and trans., *History of the Imams and Seyyids of Oman, by Salil-ibn-Razik, from A.D. 661–1856* (London, 1871); 'Abdullah ibn Humayd al-Salimi, *Tuhfat al-A'yan bi-Sirat Ahl 'Uman* (1927; rpt. edn., Kuwait, 1974); Muhammad ibn 'Abdullah al-Salimi, *Nahdat al-A'yan bi-Hurriyat 'Uman* (Cairo, n.d. [1961?]).

³⁰ James Morris, *Sultan in Oman* (London, 1957).

³¹ J. E. Peterson, *Oman in the Twentieth Century: Political Foundations of an Emerging State* (London, 1978).

³² Ranulph Fiennes, *Where Soldiers Fear to Tread* (London, 1975); Tony Jeapes, *SAS: Operation Oman* (London, 1980); John Akehurst, *We Won a War: The Campaign in Oman, 1965–1975* (Wilton, Salisbury, 1982).

Elsewhere in the Arabian Peninsula, states evolved out of tribal leadership. An eighteenth-century alliance between a petty shaykhly family of the Najd in central Arabia, the Al Sa'ud, and an itinerant Islamic reformer, Muhammad ibn 'Abd al-Wahhab, produced the means and motivation for Saudi expansion over much of Arabia during the last 200 years. Ultimately, however, that expansionism was checked, principally by encounters with the British sphere of influence. The maturation of the third Saudi state in this century, beginning with the recapture of Riyadh in 1902 and continuing through the adoption of the name of "Kingdom of Saudi Arabia" in 1932, is the most widely chronicled example of state formation in the Peninsula.

Despite its prominent position on the world stage in recent years, Saudi Arabia remains a closed and largely unknown country. The bare outlines of its history have been recounted a number of times, for instance, in the popular histories by David Holden and Richard Johns, and Robert Lacey.³³ Still, the process by which a minor family of the central Najd gained dominion over much of Arabia, lost it and regained it twice, and acquired a prominent position on the international stage has not been examined adequately. Details of the early period are scarce and often contradictory. Even the dates of the early imams (as the Al Sa'ud leaders were styled) are not clear, not least because of rival pretenders and overlapping dates. Neither Western accounts, as written by George Rentz and Bayly Winder, nor modern Saudi renditions, such as that by Khayr al-Din Zirkali, are conclusive, because of the lack of written evidence.³⁴ In the nineteenth century, any clear line of succession is clouded by the Egyptian destruction of the original Saudi capital at al-Dir'iya in 1818 and the holding of hostages in Cairo. Family rivalries and Ottoman intrigues resulted in the decline of the second Saudi state in the 1870s, with pretenders to the Al Sa'ud leadership in exile again, this time in Istanbul.

It was not until the turn of the century and the emergence of young 'Abd al-'Aziz ibn 'Abd al-Rahman that the Al Sa'ud regained control of Najd and set the foundations for the third Saudi state. The recapture of Riyadh has assumed mythological proportions in contemporary Saudi historiography and provided the stage setting for the legend of King 'Abd al-'Aziz. The first stage in 'Abd al-'Aziz's drive to restore Saudi fortunes was the reconquest of the Najd. In this and his later campaigns, 'Abd al-'Aziz was heavily dependent on the Ikhwan, the bedouin tribesmen turned into warriors of Wahhabi Islam. The relationship of the Saudi leader with the Ikhwan, as well as the question of whether he founded the movement or simply took it over, is still a matter of conjecture, as shown in the works of John Habib and Christine Helms.³⁵ By the end of the 1920s, however,

³³ David Holden and Richard Johns, with James Buchan, *The House of Saud: The Rise and Rule of the Most Powerful Dynasty in the Arab World* (New York, 1982); Robert Lacey, *The Kingdom: Arabia and the House of Saud* (London, 1981).

³⁴ George S. Rentz, "Muhammad ibn 'Abd al-Wahhab (1703/04–1792) and the Beginnings of the Unitarian Empire in Arabia" (Ph.D. dissertation, University of California, Berkeley, 1948); R. Bayly Winder, *Saudi Arabia in the Nineteenth Century* (London, 1965); Khayr al-Din al-Zirkali, *Shibh al-Jazira fi 'Ahd al-Malik 'Abd al-'Aziz*, 3d edn. (Beirut, 1985).

³⁵ John S. Habib, *Ibn Sa'ud's Warriors of Islam: The Ikhwan of Najd and Their Role in the Creation of the Saudi Kingdom, 1910–1930* (Leiden, 1978); Christine Moss Helms, *The Cohesion of Saudi Arabia: Evolution of Political Identity* (London, 1981).

the Ikhwan had turned against the Al Sa'ud, and it was only with great difficulty and British help that 'Abd al-'Aziz managed to deflect their challenge. But the antipathy of the Ikhwan and the ultra-conservative Wahhabis to the Al Sa'ud has never completely died away, as was clear during the 1979 siege of the Great Mosque in Mecca.³⁶

One factor in the disillusionment of the Ikhwan resulted from 'Abd al-'Aziz's forbidding them to continue their raids north into the British mandates of Iraq and Transjordan. Given the preeminent position of Britain in the region, it should not be surprising that 'Abd al-'Aziz's success, once he began to expand out of the interior of Arabia, was dependent on good relations with London. Since British archives not only record the diplomatic activity but also constitute one of the few extensive primary sources on Saudi history, this subject has received extensive treatment, including books by Gary Troeller, Clive Leatherdale, Jacob Goldberg, and others.³⁷

British policy toward the Al Sa'ud exhibited a curious bifurcation during this formative period, arising from a governmental division of responsibilities for the Middle East. In Cairo, the Arab Bureau represented the views of the Foreign and Colonial Offices and favored the Hashimis of al-Hijaz to lead the Arab revolt against the Ottomans, a stance popularized in the film *Lawrence of Arabia* (1962). The Gulf, however, was the preserve of the British Government of India, which viewed the Al Sa'ud as the more dynamic and viable force. The failure of His Majesty's Government to throw its weight behind the Najdis and, even more, its encouragement of a Hashimi kingdom of the Hijaz after the war, was a principal factor in the decision of H. St. John B. Philby to resign from government service and offer his services to 'Abd al-'Aziz. As a result, Philby was able to take advantage of a unique opportunity to chronicle the geography and society of great portions of Arabia.³⁸ It also provided Elizabeth Monroe, in her biography of Philby, a focal point around which to portray the rivalry within British official circles.³⁹

It seems undeniable that without 'Abd al-'Aziz, the modern Saudi state would not exist, at least not in its present form. There is no lack of published material praising the founder of the third Saudi state, both by Saudi writers such as Mohammed Almana and Prince Turki M. Saud Al Saud and by Western biographers, including Ameen Rihani and David Howarth.⁴⁰ At the same time,

³⁶ Joseph A. Kechichian, "Islamic Revivalism and Change in Saudi Arabia: Juhayman al-'Utaybi's 'Letters' to the Saudi People," *Muslim World*, 80 (1990): 1-16.

³⁷ Gary Troeller, *The Birth of Saudi Arabia: Britain and the Rise of the House of Sa'ud* (London, 1976); Clive Leatherdale, *Britain and Saudi Arabia, 1925-1939: The Imperial Oasis* (London, 1983); Jacob Goldberg, *The Foreign Policy of Saudi Arabia: The Formative Years, 1902-1918* (Cambridge, Mass., 1986).

³⁸ H. St. J. B. Philby, *Arabia of the Wahhabis* (London, 1928); *The Empty Quarter* (London, 1933); *Arabian Days* (London, 1948); *Arabian Highlands* (Ithaca, N.Y., 1952); *Arabian Jubilee* (New York, 1953); *Saudi Arabia* (London, 1955); *Forty Years in the Wilderness* (London, 1957); and *Land of Midian* (London, 1957).

³⁹ Elizabeth Monroe, *Philby of Arabia* (London, 1973).

⁴⁰ Mohammed Almana, *Arabia Unified: A Portrait of Ibn Saud* (London, 1980; rev. edn., 1982); Prince Turki M. Saud Al Saud, "The Great Achievement: King 'Abd al-'Aziz and the Founding of the Third Su'udi State, 1902-1932" (Ph.D. thesis, University of London, School of Oriental and African

the role of a single strong personality has been overemphasized. Mishary Nuaim outlines the crucial part played by merchants from the Najd, Hasa, and Hijaz in financing 'Abd al-'Aziz's early expansion, while Mohamed Elgari argues that the Hijazi merchant families were able to shape the economy and bureaucracy of the resultant state to their considerable advantage.⁴¹

Recent Saudi history aptly illustrates the predicaments inherent in hereditary succession. Under 'Abd al-'Aziz's son and chosen heir, King Sa'ud (reign 1953–1964), the kingdom drifted. Riyadh's activist policy on boundaries in the Gulf led to near disaster. In 1952, Saudi soldiers moved into al-Buraymi oasis, shared between Abu Dhabi and Oman. It heralded an unusual alliance between the conservative kingdom and Egypt's Nasser against Britain and its allies. A pro-Saudi view of the situation was published by the Arabian American Oil Company (ARAMCO), while the British side was given by J. B. Kelly.⁴² But the Riyadh-Cairo axis fell apart after the Yemeni revolution in 1962, as Gregory Gause and Saeed Badeeb explain.⁴³ Direction was restored only under King Faisal (reign 1964–1973). His relatively weak successor, King Khalid (reign 1973–1982), was replaced by the intermittently more dynamic King Fahd (reign 1982–), who has been faced with such problems as a drastic fall in income and external threats from Iran and Iraq. Just as important, all of 'Abd al-'Aziz's sons have had to contend with resistance and rivalries within the vast royal family, as is made clear in studies by Gary Samore, Alexander Bligh, and Mashaal Al Saud.⁴⁴

Meanwhile, the creation and evolution of a modern government structure went hand-in-hand with increasing political sophistication. The creation of the first council of ministers in 1953, King Faisal's inclusion of younger educated Saudis during the 1970s, and the emergence of the various ministries and departments have been examined by Charles Harrington, David Long, Summer Scott Huyette, and Arthur Young.⁴⁵ Much valuable work has been contributed by Saudi scholars, such as Tawfiq Sadiq, Soliman Solaim, Ibrahim Awaji, Hamad Al-Hamad,

Studies, 1983); Ameen Fares Rihani, *Ibn Sa'oud of Arabia: His People and His Land* (London, 1928); David Howarth, *The Desert King: A Life of Ibn Saud* (London, 1964).

⁴¹ Mishary Abdalrahman Al-Nuaim, "State Building in a Non-Capitalist Social Formation: The Dialectics of Two Modes of Production and the Role of the Merchant Class, Saudi Arabia 1902–1932" (Ph.D. dissertation, University of California, Los Angeles, 1986); Mohamed Ali Elgari, "The Pattern of Economic Development in Saudi Arabia as a Product of Its Social Structure" (Ph.D. dissertation, University of California, Riverside, 1983).

⁴² Arabian American Oil Company, *Oman and the Southern Shore of the Persian Gulf* (Cairo, 1952); J. B. Kelly, *Eastern Arabian Frontiers* (London, 1964).

⁴³ F. Gregory Gause III, *Saudi-Yemeni Relations: Domestic Structures and Foreign Influence* (New York, 1990); Saeed M. Badeeb, *The Saudi-Egyptian Conflict over North Yemen, 1962–1970* (Boulder, Colo., 1986).

⁴⁴ Gary Samuel Samore, "Royal Family Politics in Saudi Arabia, 1953–1982" (Ph.D. dissertation, Harvard University, 1983); Alexander Bligh, *From Prince to King: Royal Succession in the House of Saud in the Twentieth Century* (New York, 1984); Mashaal Abdullah Turki Al Saud, "Permanence and Change: An Analysis of the Islamic Political Culture of Saudi Arabia as It Faces the Challenges of Development with Special Reference to the Royal Family" (Ph.D. dissertation, Claremont Graduate School, 1982).

⁴⁵ Charles W. Harrington, "The Saudi Arabian Council of Ministers," *Middle East Journal*, 12 (1958): 1–19; David Edwin Long, "The Board of Grievances in Saudi Arabia," *Middle East Journal*, 27 (1973): 71–76; Summer Scott Huyette, *Political Adaptation in Sa'udi Arabia: A Study of the Council of Ministers* (Boulder, Colo., 1985); Arthur N. Young, *Saudi Arabia: The Making of a Financial Giant* (New York, 1983).

Mohammed al-Tawail, Motleb Nafisa, and Othman Rawaf, some of whom now occupy senior positions.⁴⁶ In a related arena, Fayez Badr dissects the evolution of development planning.⁴⁷

Fouad al-Farsy provides a comprehensive survey of the kingdom's social and economic development, while reflections from different viewpoints on the more fundamental prospects and problems brought by oil income are provided by Fatina Shaker and John Shaw and David Long.⁴⁸ The country's wealth, regional prominence, and secrecy have provoked ample criticism, whether from Western conservative critics such as J. B. Kelly, Western leftists such as Helen Lackner, or virulent internal opponents such as Nasir al-Sa'id.⁴⁹

The smaller states of the Gulf have evolved from origins similar to that of Saudi Arabia. An additional key factor in their modern history, however, involves the consolidation of British influence in the Gulf, particularly in the nineteenth century, through the exclusion of European (and Ottoman) rivals. This process is the focus of Kelly's immense treatise in imperial history.⁵⁰ Kelly also provides an explanation of the system of British representation in the Gulf.⁵¹ The rise of the British position more or less coincided with the emergence of certain tribes along the Gulf littoral. As Ahmad Abu Hakima shows, the Al Sabah and the Al Khalifa derive from well-pedigreed tribes of the Najd that found their way to Kuwait and Bahrain respectively in the mid-eighteenth century.⁵² The preeminent families in Kuwait essentially formed a partnership whereby the Al Sabah took responsibility for providing governance while the others pursued commerce.

This balance was disturbed by two factors. Shaykh Mubarak (reign 1896–1915), in addition to keeping Kuwait out of the clutches of the Ottomans by signing a treaty of protection with the British in 1899, consolidated his own power. As

⁴⁶ Muhammad Tawfiq Sadiq, *Tatawwur al-Hukm wal-Idara fi al-Mamlaka al-'Arabiya al-Sa'udiya* (Riyadh, 1965); Soliman A. Solaim, "Constitutional and Judicial Organization in Saudi Arabia" (Ph.D. dissertation, Johns Hopkins University, 1970); Ibrahim Mohamed Awaji, "Bureaucracy and Society in Saudi Arabia" (Ph.D. dissertation, University of Virginia, 1971); Hamad Sadun Al-Hamad, "The Legislative Process and the Development of Saudi Arabia" (Ph.D. dissertation, University of Southern California, 1973); Mohammed Abdulrahman al-Tawail, "Institute of Public Administration in Saudi Arabia: A Case Study in Institution Building" (Ph.D. dissertation, West Virginia University, 1974); Motleb Abdullah Nafisa, "Law and Social Change in Muslim Countries: The Concept of Islamic Law Held by the Hanbali School and the Saudi Arabian Legal System" (S.J.D. thesis, Harvard Law School, 1975); and Othman Yasin al-Rawaf, "The Concept of the Five Crises in Political Development: Relevance to the Kingdom of Saudi Arabia" (Ph.D. dissertation, Duke University, 1980).

⁴⁷ Fayez Ibrahim Badr, "Developmental Planning in Saudi Arabia: A Multidimensional Study" (Ph.D. dissertation, University of Southern California, 1968).

⁴⁸ Fouad al-Farsy's regularly updated book, *Modernity and Tradition: The Saudi Equation* (London, 1991); Fatina Amin Shaker, "Modernization of the Developing Nations: The Case of Saudi Arabia" (Ph.D. dissertation, Purdue University, 1972); John A. Shaw and David E. Long, *Saudi Arabian Modernization: The Impact of Change on Stability* (New York, 1982).

⁴⁹ J. B. Kelly, *Arabia, the Gulf, and the West: A Critical View of the Arabs and Their Oil Policy* (London, 1980); Helen Lackner, *A House Built on Sand: A Political Economy of Saudi Arabia* (London, 1978); Nasir al-Sa'id, *Tarikh Al Sa'ud* (1404; n.p., 1984–85).

⁵⁰ J. B. Kelly, *Britain and the Persian Gulf, 1795–1880* (Oxford, 1968).

⁵¹ J. B. Kelly, "The Legal and Historical Basis of the British Position in the Persian Gulf," in *St. Antony's Papers*, 4 (London, 1959): 119–40. See also Herbert J. Liebesny, "British Jurisdiction in the States of the Persian Gulf," *Middle East Journal*, 3 (July 1949): 330–32.

⁵² Ahmad Mustafa Abu Hakima, *History of Eastern Arabia, 1750–1800: The Rise and Development of Bahrain and Kuwait* (Beirut, 1965).

Naseer Aruri, Ahmad Baz, Suhail Shuhaiber, and Jill Crystal show, ever since this autocracy replaced the Kuwaiti oligarchy, subsequent Al Sabah rulers have been unwilling to restore the balance.⁵³ Their dominant position was considerably enhanced by the discovery of oil and the collection of oil payments by the ruling family. The initial ministries and public services departments functioned as little more than fiefdoms of close relatives of the ruler. The intrusion of currents of Arab nationalism from Nasserist Egypt and revolutionary Iraq renewed demands from various sectors of Kuwaiti society for more participation. The elected National Assembly, created soon after independence in 1961, seemed to satisfy some of these aspirations, but the path toward democracy has been tortuous, as shown in the work of Abdo Baaklini, Nicolas Gavrielides, and my own study: the Assembly was suspended in 1976 and again in 1986.⁵⁴

The development of Bahrain as a modern state was more complicated. To begin with, the Al Khalifa and their Najdi tribal allies arrived as conquerors. Their control over the more numerous Baharina (indigenous Shi'i Arabs), as well as the smaller communities of ethnic Persians and *hawala* (families that have immigrated from the Persian coast but claim Arab origins) has bedeviled Bahraini politics and social relations ever since.⁵⁵ Although indigenous Shi'i communities, mostly but not entirely Baharina, exist in various places along the Arab littoral of the Gulf, scholars have given them very little attention. F. S. Vidal's study of al-Hasa, the oasis complex of eastern Saudi Arabia, is one exception, as is James Bill's article.⁵⁶

The lack of checks and balances that allowed some members of the ruling family to run roughshod over other Bahrainis forced British intervention in internal affairs. Talal Farah describes the British pattern of intervention during the late nineteenth century, while Mahdi Tajir takes up the story in this century, including the forced abdication of Shaykh 'Isa ibn 'Ali (reign 1869–1923) and then the appointment of Charles Belgrave as adviser to the ruler.⁵⁷ Belgrave's long stay (as chronicled in his autobiography) became a key issue in the emerging tension between the Al Khalifa and an increasingly organized and radicalized opposition.⁵⁸ Under an indifferent ruler, Belgrave virtually assumed all the reins of administration. By the 1950s, when Bahrain had developed a complex

⁵³ Naseer H. Aruri, "Kuwait: A Political Study," *Muslim World*, 60 (October 1970): 321–43; Ahmed Abdullah Saad Baz, "Political Elite and Political Development in Kuwait" (Ph.D. dissertation, George Washington University, 1981); Suhail K. Shuhaiber, "Political Development in Kuwait: Continuity and Change in an Arab Independent Gulf State" (Ph.D. thesis, St. Catherine's College, University of Oxford, 1981); Jill Crystal, *Oil and Politics in the Gulf: Rulers and Merchants in Kuwait and Qatar* (Cambridge, 1990).

⁵⁴ Abdo I. Baaklini, "Legislatures in the Gulf Area: The Experience of Kuwait, 1961–1976," *International Journal of Middle East Studies*, 14 (1982): 359–79; Nicolas Gavrielides, "Tribal Democracy: The Anatomy of Parliamentary Elections in Kuwait," in Linda L. Layne, ed., *Elections in the Middle East: Implications of Recent Trends* (Boulder, Colo., 1987), 153–213; J. E. Peterson, *The Arab Gulf States: Steps toward Political Participation* (New York, 1988).

⁵⁵ See M. G. Rumaihi, *Bahrain: Social and Political Change since the First World War* (London, 1976); and Fuad I. Khuri, *Tribe and State in Bahrain: The Transformation of Social and Political Authority in an Arab State* (Chicago, 1980).

⁵⁶ F. S. Vidal, *The Oasis of Al-Hasa* (New York, 1955); James A. Bill, "Resurgent Islam in the Persian Gulf," *Foreign Affairs*, 63 (Fall 1984): 108–27.

⁵⁷ Talal 'oufic Farah, *Protection and Politics in Bahrain, 1869–1915* (Beirut, 1985); Mahdi Abdalla al-Tajir, *Bahrain 1920–1945: Britain, the Shaikh and the Administration* (London, 1987).

⁵⁸ Charles Dalrymple Belgrave, *Personal Column* (London, 1960).

government and an organized opposition movement, he had become a focus of popular discontent.

The Suez crisis of 1956 brought political tensions in Bahrain to a head. Saeed Hashim shows how Arab politics influenced opposition movements in the Gulf.⁵⁹ After a demonstration deteriorated into riots, seven opposition leaders were jailed, four of them in exile on St. Helena Island. Bernard Burrows, the Political Resident in the Persian Gulf at the time, provides an explanation and apologia for Britain's role in these events.⁶⁰ Given this history of tensions, it seems a bit surprising that Bahrain, like Kuwait, established an elected National Assembly after independence. But as Emile Nakhleh recounts, it was suspended within several years.⁶¹ These tensions were exacerbated by Iranian interference in the 1980s and by the virtual exhaustion of Bahrain's small oil reserves; the amirate's attempt to diversify as a regional banking center suffered from the post-1986 economic recession throughout the Gulf.

The Qatar Peninsula served as the springboard for the Al Khalifa's descent on Bahrain. A small population and paucity of resources ensured Qatar's place on the margin of history until the discovery of oil. The dominant Al Thani family, which rose to prominence early in the twentieth century, has very nearly turned the small state into a family enterprise. The pattern of this development has been described well by Jill Crystal and Rosemarie Said Zahlan, while Yousof Abdulla concentrates on the relationship with the protecting power.⁶² One aspect of the social transformation of the country was the move of the indigenous labor force from herding and fishing to salaried work as oil company employees. Their stories have been recorded by Nasser al-Othman.⁶³

Until independence in 1971, the United Arab Emirates (UAE) was known as the Trucial Coast or Trucial Oman. This name, signifying the accession of the littoral shaykhs to the Perpetual Treaty of Maritime Peace (1853), replaced an earlier European designation as the Pirate Coast. While the Gulf was home to some genuine freebooters in the eighteenth and early nineteenth centuries, the accusations of piracy were principally aimed at the al-Qasimis, and several British expeditions were directed at Ra's al-Khayma and other Qasimi strongholds. It has fallen to the present ruler of Sharjah, Shaykh Sultan ibn Muhammad al-Qasimi, to refute the charge against his ancestors.⁶⁴

Since then, the balance of power along the Trucial Coast has shifted south. Frauke Heard-Bey explains how, at the turn of this century, Shaykh Zayid ibn

⁵⁹ Saeed Khalil Hashim, "The Influence of Iraq on the Nationalist Movements of Kuwait and Bahrain, 1920–1961" (Ph.D. thesis, University of Exeter, 1984).

⁶⁰ Bernard Burrows, *Footnotes in the Sand: The Gulf in Transition, 1953–1958* (Wilton, Salisbury, 1990).

⁶¹ Emile A. Nakhleh, *Bahrain: Political Development in a Modernizing Society* (Lexington, Mass., 1976).

⁶² Crystal, *Oil and Politics*; Rosemarie Said Zahlan, *The Creation of Qatar* (London, 1979); Yousof Ibrahim Al-Abdulla, *A Study of Qatari-British Relations, 1914–1945* (Doha, Qatar, 1981).

⁶³ Nasser al-Othman, *With Their Bare Hands: The Story of the Oil Industry in Qatar*, Ken Whittington, trans. and ed. (London, 1984). For a more scholarly view, see Ian Seccombe and Richard Lawless, "The Gulf Labour Market and the Early Oil Industry: Traditional Structures and New Forms of Organisation," in R. I. Lawless, ed., *The Gulf in the Early 20th Century: Foreign Institutions and Local Responses* (Durham, 1986), 91–124.

⁶⁴ Sultan ibn Muhammad al-Qasimi, *The Myth of Arab Piracy in the Gulf* (London, 1986; 2d edn., 1988).

Khalifa (reign 1855–1909) enhanced and expanded a web of tribal allegiances and coalitions to create a large and powerful “state” in Abu Dhabi.⁶⁵ At the same time, the Al Maktum were beginning to develop nearby Dubai as a regional entrepot. Dubai’s prosperity predated its income from oil, but oil wealth was responsible for completing Abu Dhabi’s rise to predominance along the Trucial Coast. As Ali Mohammed Khalifa shows, that wealth enabled Abu Dhabi to assume the leadership of the newly independent UAE.⁶⁶ But it is a fragile leadership that has left the seven members of the UAE jealous of their autonomy in a number of spheres.

The UAE owes its birth to Britain’s withdrawal from the Gulf in 1971, when all the amirates but Kuwait received their independence (Kuwaiti independence came in 1961). While physical withdrawal involved little more than the removal of a few thousand troops, it also marked the end of an era of British predominance in the Gulf and responsibility for the integrity of the states under its protection, as shown by Glen Balfour-Paul.⁶⁷ International concern about the future of the Gulf, given the smallness and vulnerability of the oil states, gave rise to the topic of Gulf security, but security was a serious concern well before the 1970s.⁶⁸ John Duke Anthony and ‘Abdullah Taryam disclose how the announcement in 1968 of impending withdrawal led to a flurry of negotiations over unity.⁶⁹ In the end, Bahrain and Qatar chose to go their own way, leaving only the smaller Trucial States to join in the UAE.

The discovery of oil and the subsequent influx of income undoubtedly has been the key event in the recent history of the six oil-producing monarchies. Looking at Saudi Arabia and the Gulf states prior to World War II, it would have been very difficult to foresee the magnitude of change brought about by oil. The original oil concessions gave the major British and American companies the right to exploit the oil beneath entire countries on payment of small royalties and minimal taxes, as the studies by Olaf Caroe and Stephen Longrigg show.⁷⁰ It would be years before oil-producing states took increasing control of their resources, began a gradual policy of nationalization of producing companies, created their own national companies, and acquired the clout that made OPEC a force to be feared in the 1970s. This story, ably told by Steven Schneider, Ian Skeet, and Daniel Yergin, extends far beyond the confines of the present essay.⁷¹ A view from the

⁶⁵ Frauke Heard-Bey, *From Trucial States to United Arab Emirates: A Society in Transition* (London, 1982). See also Muhammad Morsy Abdullah, *The United Arab Emirates: A Modern History* (New York, 1978); and Rosemarie Said Zahlan, *The Origins of the United Arab Emirates: A Political and Social History of the Trucial States* (London, 1978).

⁶⁶ Ali Mohammed Khalifa, *The United Arab Emirates: Unity in Fragmentation* (Boulder, Colo., 1979).

⁶⁷ Glen Balfour-Paul, *The End of Empire in the Middle East: Britain’s Relinquishment of Power in Her Last Three Arab Dependencies* (Cambridge, 1991).

⁶⁸ J. E. Peterson, *Defending Arabia* (London, 1986).

⁶⁹ John Duke Anthony, *Arab States of the Lower Gulf: People, Politics, Petroleum* (Washington, D.C., 1975); ‘Abdullah Omran Taryam, *The Establishment of the United Arab Emirates, 1950–85* (London, 1987).

⁷⁰ Olaf Caroe, *The Wells of Power: The Oilfields of South-Western Asia* (London, 1951, 1976); Stephen H. Longrigg, *Oil in the Middle East: Its Discovery and Development*, 3d edn. (London, 1968).

⁷¹ Steven A. Schneider, *The Oil Price Revolution* (Baltimore, Md., 1983); Ian Skeet, *OPEC: Twenty-Five Years of Prices and Politics* (Cambridge, 1988); Daniel Yergin, *The Prize: The Epic Quest for Oil, Money, and Power* (New York, 1991).

Gulf by Farouk Akhdar casts a strongly critical eye on ARAMCO's role in Saudi society and politics.⁷²

Another effect of oil was the creation of triangular relationships between host countries, Western oil companies, and Western governments. The winning of the Saudi concession by what was to become ARAMCO proved to be an opening step in the lessening of the Al Sa'ud's dependence on Britain and the creation of a "special relationship" with the United States, as pointed out by Irvine H. Anderson.⁷³ Husain Albaharna and J. C. Wilkinson explain how the Anglo-American commercial and political rivalry over control of oil resources also embroiled the two allies in boundary disputes, in al-Buraymi and elsewhere.⁷⁴

The burgeoning of oil production in the Peninsula, shortly followed by the tremendous process of socioeconomic transformation, essentially began in the late 1940s (although Bahrain's production dates from the 1930s). J. P. Bannerman and Ibrahim Al-Elawy show how oil production rapidly altered the social structure of tribes and communities and induced labor migration and a decline in traditional economies.⁷⁵ Gary Anderson details the process of change in rural areas and urbanization in eastern Saudi Arabia, while Soraya Altorki and Donald Cole use a Najdi town as a case study.⁷⁶ Taha El-Farra, Mohammed al-Fiar, and Mohammed Ebrahim explain why planned bedouin sedentarization projects were unsuccessful; Abdulrasoul Moosa reveals continuing problems of assimilation in Kuwait.⁷⁷ The larger process of social change as a result of the gradual penetration of outside influences is the subject of Mohammad Mutawa, and Jacqueline Ismael uses dependency theory to demonstrate the linkage between internal change and external influence.⁷⁸

The emergence of a new middle class in Arabia, as elsewhere in the Middle East and Third World, has been the subject of considerable conjecture. Stephen

⁷² Farouk M. H. Akhdar, "Multinational Firms and Developing Countries: A Case Study of the Impact of the Arabian American Oil Co. (ARAMCO) on the Development of the Saudi Arabian Economy" (Ph.D. dissertation, University of California, Riverside, 1974).

⁷³ Irvine H. Anderson, *Aramco, the United States, and Saudi Arabia: A Study of the Dynamics of Foreign Oil Policy, 1933-1950* (Princeton, N.J., 1981).

⁷⁴ Husain M. Albaharna, *The Arabian Gulf States: Their Legal and Political Status and Their International Problems*, 2d rev. edn. (Beirut, 1975); J. C. Wilkinson, *Arabia's Frontiers: The Story of Britain's Blue and Violet Lines* (London, 1991).

⁷⁵ J. P. Bannerman, "The Impact of the Oil Industry on Society in the Arabian Peninsula," in Lawless, *The Gulf in the Early 20th Century*, 76-90; Ibrahim S. Al-Abdullah Al-Elawy, "The Influence of Oil upon Settlement in Al-Hasa Oasis, Saudi Arabia" (Ph.D. thesis, University of Durham, 1976).

⁷⁶ Gary Anderson, "Differential Urban Growth in the Eastern Province of Saudi Arabia: A Study of the Historical Interaction of Economic Development and Socio-Political Change" (Ph.D. dissertation, Johns Hopkins University, 1985); Soraya Altorki and Donald P. Cole, *Arabian Oasis City: The Transformation of 'Unayzah* (Austin, Tex., 1989).

⁷⁷ Taha Osman M. El-Farra, "The Effects of Detribalizing the Bedouins on the Internal Cohesion of an Emerging State, The Kingdom of Saudi Arabia" (Ph.D. dissertation, University of Pittsburgh, 1973); Mohammed Hussein al-Fiar, "The Faisal Settlement Project at Haradh, Saudi Arabia: A Study in Nomad Attitudes toward Sedentarization" (Ph.D. dissertation, Michigan State University, 1977); Mohammed Hossein Saleh Ebrahim, "Problems of Nomad Settlement in the Middle East: With Special Reference to Saudi Arabia and the Haradh Project" (Ph.D. dissertation, Cornell University, 1981); Abdulrasoul A. Al-Moosa, "Bedouin Shanty Settlements in Kuwait: A Study in Social Geography" (Ph.D. thesis, University of London, School of Oriental and African Studies, 1976).

⁷⁸ Mohammad A. J. al-Mutawa, "Social Change and Political Development in the Omani Coast 1934-1970" (Ph.D. thesis, University of Reading, 1983); Jacqueline S. Ismael, *Kuwait: Social Change in Historical Perspective* (Syracuse, N.Y., 1982).

Duguid uses the example of an early Saudi oil minister to analyze it.⁷⁹ Other observers, such as William Rugh, Mark Heller and Nadav Safran, Mordechai Abir, and myself have also discerned the makings of a middle class, although its size and development are a matter of debate.⁸⁰ Muhammed Rumaihi points out that problems of dislocation, family relations, restrictions on women, and dependence on welfare states still remain.⁸¹ Oil has had far-reaching, if indirect, consequences for the Yemens, through foreign aid from the oil states, remittance flows from workers in the Gulf, and a transformation of those workers' social and political attitudes. Yemen's problems are more typical of Third World countries, as Mohamed Said el Attar's early analysis reveals.⁸²

The record of historical exploration of the Arabian Peninsula is replete with paradoxes.⁸³ On the one hand, it displays an ancient pedigree; on the other, scholarly attention is the product of several decades only. While scholarly writing about the Peninsula is meager compared to that for Egypt or Iran, trendy subjects such as Gulf security (and the war for Kuwait) generate a flood of uneven and largely unremarkable publications. Some writers emphasize the extent and pace of change; others stress the continuity of tradition. The outlines of Arabia's modern history are well known. It is the underlying firmament that remains *terra incognita*. The exploration of that territory remains necessary to sort out the correct balance between change and continuity and to reach reliable conclusions about the nature of state formation in the Arabian Peninsula.

⁷⁹ Stephen Duguid, "A Biographical Approach to the Study of Social Change in the Middle East: Abdullah Tariki as a New Man," *International Journal of Middle East Studies*, 1 (July 1970): 195–220.

⁸⁰ William A. Rugh, "Emergence of a New Middle Class in Saudi Arabia," *Middle East Journal*, 27 (1973): 7–20; Mark Heller and Nadav Safran, *The New Middle Class and Regime Stability in Saudi Arabia* (Cambridge, Mass., 1985); Mordechai Abir, *Saudi Arabia in the Oil Era: Regime and Elites—Conflict and Collaboration* (Boulder, Colo., 1988); J. E. Peterson, "Change and Continuity in Arab Gulf Society," in Charles Davies, ed., *After the War: Iraq, Iran and the Arab Gulf* (Chichester, West Sussex, 1990), 287–312.

⁸¹ Muhammed Rumaihi, *Beyond Oil: Unity and Development in the Gulf*, James Dickins, trans. (London, 1986).

⁸² Mohamed Said el Attar, *Le Sous-développement économique et social du Yemen* (Algiers, 1964).

⁸³ It would be amiss to conclude without mentioning a number of collections and conference proceedings that should not go overlooked: Derek Hopwood, ed., *The Arabian Peninsula: Society and Politics* (London, 1972); Alvin J. Cottrell, gen. ed., *The Persian Gulf States: A General Survey* (Baltimore, Md., 1980); Paul Bonnenfant, ed., *La Péninsule arabique d'aujourd'hui*, 2 vols. (Paris, 1982); Robert W. Stookey, ed., *The Arabian Peninsula: Zone of Ferment* (Stanford, Calif., 1984); Ian Richard Netton, ed., *Arabia and the Gulf: From Traditional Society to Modern States; Essays in Honour of M. A. Shaban's 60th Birthday (16th November 1986)* (London, 1986); and the papers of annual symposia of the University of Exeter's Centre for Arab Gulf Studies (all published in London): Tim Niblock, ed., *Social and Economic Development in the Arab Gulf* (1980), Tim Niblock, ed., *State, Society, and Economy in Saudi Arabia* (1982), B. R. Pridham, ed., *Contemporary Yemen: Politics and Historical Background* (1984), and B. R. Pridham, ed., *Oman: Economic, Social and Strategic Developments* (1987).

A Historiographic Review of Literature on the Origins of the Arab-Israeli Conflict

KENNETH W. STEIN

A RECENTLY COMPLETED STUDY analyzed the status of Middle Eastern history in colleges and universities in the United States. It revealed that American and European historians have maintained an imposing preponderance within academic departments of history. Curriculum and scholarship about "Third World" countries continue to be represented by proportionately few historians. Furthermore, within the "Third World" area, there are fewer teaching positions for the Middle East than for Africa, East Asia, or Latin America.¹ In North America and Western Europe, academic centers for the study of the Middle East developed slowly after World War II. Studying and teaching Middle Eastern history took place at a limited number of institutions, introduced only as adjuncts to the coursework on Semitic languages, philology, or religion. Today, the number of properly trained Middle Eastern historians remains low, partly because of the challenging requirements of intricate foreign languages, the prolonged time needed to complete a doctorate, and lower remuneration than in alternative job opportunities for those with a Middle Eastern academic interest. In comparison to other fields of historical study, Middle Eastern history is a relatively young specialty, sparsely populated, and undeveloped in range and depth of scholarly publications.

Since Middle Eastern historians are few in number, they are usually responsible for teaching regional courses that require broad historical coverage and often encompass issues that range chronologically from the Prophet Muhammad's life to Ayatollah Khomeini's death. Unlike academics teaching American or European history, Middle Eastern historians are generally not able to focus on considerably narrower geographic areas, themes, or time periods (such as the "Old South," "American Popular Culture," or the "Jacksonian Period"). Anecdotal evidence

Space requirements have forced rigorous selectivity in mentioning authors and works in this essay. Therefore, I have focused only on publications devoted exclusively to the period before 1950. Some of the material used in writing this essay appeared in an earlier publication, "A General Historiographic and Bibliographic Review of Literature on Palestine and the Palestinian Arabs," *Orient*, 22 (March 1981): 100–12. I would like to thank the editors of *Orient* for granting permission to use portions of that article in this essay. My deep appreciation is extended to Margaret Eisenband, who worked diligently as the research assistant for this project. In addition, I would like to thank Yosef Gorny, Bruce Maddy-Weitzman, Philip Mattar, Jehuda Reinharz, and Bernard Wasserstein for their ideas and suggestions in framing this essay. Responsibility for the contents is strictly my own.

¹ Kenneth W. Stein, "The Study of Middle Eastern History in the United States," *Jerusalem Quarterly*, 46 (Spring 1988): 49–64.

indicates that most historians of Europe and America seem to write only about their scholarly specialty, and relatively few of them publish regular contributions on a broader historical theme. But for those who teach modern Middle Eastern history, primary scholarship shares the stage with writing on the Arab-Israeli conflict. The data show that one-third of the articles and books written between 1962 and 1986 spotlight some aspect of the Arab-Israeli conflict. Of the 4,553 English publications indexed by *Historical Abstracts* as focusing on all topics of modern Middle Eastern history, more than 1,800 are about some facet of the Arab-Israeli conflict.² The Arab-Israeli conflict has been the one issue that consistently generates publications.

A closer analysis of the publications indexed and surveyed reveals that more than 90 percent of what was written about the evolution of the Arab-Israeli conflict focused on the period after 1950. These publications deal with subjects such as modern Israel, the Palestine Liberation Organization, the 1967 and 1973 wars, aspects of the Arab-Israeli negotiating process, and most recently the Palestinian *intifada*. Based on the index in *Historical Abstracts*, from 1980 through 1986 more than 450 articles were written exclusively about modern Israel; during the same period, only 150 articles center on such topics as Zionism, the British mandate, and Palestinian Arab nationalism. Between 1962 and 1972, a total of 12 articles was published in English about the Palestine mandate, and from 1972 to 1986 an average of only 15 articles were published per year. Only 5 to 7 percent of publications about the modern Middle East written in English and indexed in *Historical Abstracts* deal with the time frame of late Ottoman Palestine to 1950, which for the purpose of this essay is considered the historical origin of the conflict. Comparatively few scholarly works have been written in English about historical aspects of the emerging Arab-Israeli conflict. Much of the finest work has been produced by Israeli scholars and was written in the last twenty-five years, with a considerable portion of it published only in Hebrew.³ Histories written in

² Stein, "Study of Middle Eastern History," 61. Data culled from 1986 through 1988, also indexed by *Historical Abstracts*, suggest a similar finding: one-third of scholarly publications continued to focus on an aspect of the Arab-Israeli conflict, and very few concentrated on the pre-1950 period.

³ A mere handful of bibliographic or historiographic essays on topics associated with the conflict's origins have previously appeared. However, several bibliographic indexes and annotated bibliographies are particularly valuable for those with an interest in the conflict's origins. For journal and periodical references covering the period up to 1930, two reference works are especially useful: Peter Thomsen, *Die Palästina-Literatur*, 6 vols. (Leipzig, 1911); and Stuart C. Dodd, gen. ed., *A Post-War Bibliography of the Near Eastern Mandates* (Beirut, 1932-). Dodd's massive multivolume bibliographic compilation covers publications in six languages between November 1918 and January 1930. For the years at the end of the mandate, Sophie A. Udin's edition of *Palestine and Zionism: A Three Year Cumulation, January 1946–December 1948* (New York, 1949), is a very useful bibliography of periodicals, books, pamphlets, reprints, and ephemera published in English, Hebrew, Yiddish, and other languages.

A general introduction to Zionist historiography is provided by Israel Kolatt, "Reflections on the Historiography of Zionism and the Yishuv," in Lee I. Levine, ed., *Jerusalem Cathedral*, Vol. 1: *Studies in the History, Archaeology, Geography and Ethnography of the Land of Israel* (Jerusalem, 1981), 314–27. For Zionism, the most worthwhile selected bibliographies are found in each of David Vital's three volumes published by Oxford: *The Origins of Zionism* (1975), *Zionism: The Formative Years* (1982), and *Zionism: The Crucial Phase* (1987). The journal *Zionism*, which since 1981 has been *Studies in Zionism*, publishes annually an extensive bibliography of recent works on Zionism that includes other topics associated with the historical origins of the conflict.

Four historiographic essays on the Palestinian community are important: Tarif Khalidi, "Palestinian Historiography," *Journal of Palestine Studies*, 10 (Spring 1981): 59–76; Walid Khalidi and Jill

English have sometimes been compiled by journalists, publicists, or scholars generally not trained as Middle Eastern historians.

Histories about the origins of the Arab-Israeli conflict are heavily influenced by the period in which they are written, the tendency of many authors to skew their findings in order to idealize, editorialize, or legitimize a parochial viewpoint, and the availability and use of archival material. For this analysis, histories about the conflict's origins will be divided into three periods: pre-1950, post-1950 (especially after the June 1967 War), and the mid-1980s to the present.

WITH FEW EXCEPTIONS, histories written before 1950 (immediately after Israel's establishment and the creation of the Palestinian diaspora) tend to be ideologically orthodox, sycophantic, or self-centered. Most are neither objective assessments nor functional studies based on primary source material. Most are firsthand analyses or impressionistic accounts of Palestine's inherited Ottoman legacy, the evolution and development of Zionism, the British involvement in the Middle East in general, and the Arab community living in Palestine.

Before 1950, books and pamphlets on Zionism and the Jewish presence in Palestine were exponentially more plentiful than histories about either the British administration or the Arab community. In contrast to limited accounts of the Palestinian Arab community, a long period of Zionist historiography predated the conflict's development in Palestine. Most of the histories and analyses of Zionism written around the turn of the century relate its meaning to Europe and European political philosophies. Zionism defined and refined itself as the Jewish national concept, with increasing emphasis on the historic tie to *Eretz Yisrael* (the Land of Israel) or modern Palestine. There were literally hundreds of books written about Zionism's genesis and evolution, but only a few endured as classics: Nahum Sokolow's *History of Zionism, 1600–1918* (1919), Leonard J. Stein's *Zionism* (1925), and Adolf Boehm's *Die zionistische Bewegung* (1935–1937).

Other authors of Zionist history dealing primarily with the Jewish presence in *Eretz Yisrael* explain Jewish physical and demographic growth as necessary for fulfillment of the ancient Jewish dream to return from forced exile to the ancestral land. They combine this religious-philosophical imperative with Zionism's practical contributions to Palestine's role in strategic considerations of the Great Powers. In addition to portraying Zionism as just and righteous, many writers highlight the positive contributions of Jewish growth to the indigenous Arab population: assisting Palestine's ailing agricultural economy and drastically reducing Arab mortality rates through better health care. Many writers remind the British that Jewish revenue was essential for their administration of Palestine. Scientific arguments justifying Jewish development are used in the works of Moshe Burstein, David Horowitz and Rita Hinden, Abraham Granovsky, Abra-

Khadduri, eds., *Palestine and the Arab-Israeli Conflict: An Annotated Bibliography* (Beirut, 1974); Yehoshua Porath, "Palestinian Historiography," *Jerusalem Quarterly*, 5 (Fall 1977): 95–104 (especially enlightening because of the Arabic sources it evaluates); and Stein, "General Historiographic and Bibliographic Review of Literature on Palestine," 100–12. It covers not only the period up to 1948 but also the historiography of the Palestinians and the PLO until 1980.

ham Revusky, and Arthur Ruppin.⁴ The best work detailing social and economic problems within the Arab community and Zionism's salutary impact on it appears in the twelve articles in Enzo Sereni and R. E. Ashery's edition of *Jews and Arabs in Palestine: Studies in a National and Colonial Problem* (1936, 1976).

Most Zionist memoirs from the mandate period contain an underlying tone of relentless commitment to sustain and enlarge the Jewish presence in Palestine. The Holocaust reaffirmed attention to Jewish revival while the Jewish national home was developing. After escaping from East European oppression, Zionist writers who had lived through two world wars, the Holocaust, and the creation of the state of Israel were emboldened by their survival and success. They tell a story of courage, pioneering spirit, and collective heroism. The personal writings of Zionists who shaped the Jewish state reveal that these events changed their outlook about themselves, the British, and the Arabs.⁵

British authors who wrote in this pre-1950 period try to explain whether the Palestine mandate and the development of the Jewish national home were good or bad for the British empire, for the Zionists, or for the Arabs in the Middle East in general. British officials who served in either the military (1918–1920) or civilian administration (1920–1948) of His Majesty's Government offer personal accounts and recollections about the mandate's legitimacy, operation, and policy shortcomings.

The most painstaking juristic examination and comprehensive analysis of the mandate text may be found in J. Stoyanovsky's *Mandate for Palestine* (1928). In a stunningly blunt manner, H. J. Simson, in *British Rule and Rebellion* (1937), condemns Britain's failure to use sufficient force to end the Arab rebellion in 1936. Several people who worked in the Palestine administration or for Jewish organizations have written books that define how the mandate functioned, but their renditions are handicapped by the absence of perspective and a tendency to touch up the record of their own performance. These include Norman Bentwich's *England in Palestine* (1932) and *Fulfilment in the Promised Land* (1938, 1976). Much more critical of political Zionism and praiseworthy of Britain's stewardship is Albert Hyamson's *Palestine: A Policy* (1942) and *Palestine under the Mandate, 1920–1948* (1950, 1976). The most prolific author in the Palestine administration was Douglass V. Duff, who served in the Palestine police force and published more than a dozen books. His autobiography, *May the Winds Blow!*

⁴ See Moshe Burstein, *Self-Government of the Jews in Palestine since 1900* (1934; rpt. edn., Westport, Conn., 1976); David Horowitz and Rita Hinden, *Economic Survey of Palestine* (Tel Aviv, 1938); Abraham Granovsky, *Land Policy in Palestine* (New York, 1940); Abraham Revusky, *Jews in Palestine* (New York, 1936); Arthur Ruppin, *Der Aufbau des Landes Israel* (Berlin, 1919).

⁵ Perhaps the most revealing memoirs of Zionist leaders during the formative period of Jewish settlement are by Frederick H. Kisch, *Palestine Diary* (1938; rpt. edn., New York, 1974); and Chaim Arlosoroff, *Yoman Yerushalayim* [Palestine Diary] (Tel Aviv, 1949). Kisch was chairman of the Palestine Zionist Executive, and Arlosoroff was chairman of its successor organization, the Jewish Agency. Both demonstrated that their ability to compromise with British authorities contributed significantly to the accessibility of Zionist leaders to British decision-makers. See also Yosef Haim Brenner, *Igrot* [Letters] (Tel Aviv, 1941); Eliyahu Golomb, *Hevyon Oz* [Hidden Strength] (Tel Aviv, 1950–53); David Ben-Gurion, *Zichronot* [Memoirs], vols. 1–5 (Tel Aviv, 1971–72); Zev Jabotinsky, *Ketavim* [Writings], vols. 1–8 (Jerusalem, 1947–59); Moshe Sharett, *Yoman Medini* [Diaries], vols. 1–5 (Tel Aviv, 1968–1974); Abraham Menahem Ussishkin, *Sefer Ussishkin*, R. Binyamin, ed. [Ussishkin's Book] (Jerusalem, 1934).

(1948), is a revealing glimpse of the attitudes and passions felt by those who served the British government in Palestine. The best personal recollection of administrative service in Palestine remains Ronald Storrs's *Orientalisms* (1937). This lengthy work by the military governor and later district commissioner of Jerusalem provides an incisive rendition of Britain's problems of governance during and after World War I in the early attempt to balance Arab and Zionist aspirations and fears.

Historical surveys of the region by Arab historians generally extol the virtues of the Arab cause, blame the British and Zionists for almost every evil that befell the Palestinians, and minimally analyze the nature of the Arab community in Palestine. Before 1950, Arab histories that treated Palestine or the Palestinian Arabs as separate historical entities were extremely rare. For several reasons, Palestinian Arabs authored few histories during the first period: the community had a limited number of literate writers to chronicle the unique and emerging historical consciousness, Palestinians found their cause and history subsumed into broader anti-colonial writings of Arab neighbors, Palestinian intellectuals focused on preserving their physical existence, and consensus was delayed by leadership rivalries and sociological differences within the Palestinian community. Palestinians in the 1930s and 1940s watched their society disintegrate. The few histories that were written glorify the Palestinian Arab cause and fault others for the failure to confront and retard Zionism's development. When a general Arab history includes mention of the Palestinians, the reference is usually confined to the narrow perspective of assigning blame to the British and to the Zionists for the traumatizing loss of territories. These works focus on either the interrelationships between Palestinian Arabs and larger political issues or regimes or else on a locality or region within the area that later became geographic Palestine.

Only a few early works concentrate exclusively on defining the needs and articulating the hopes of the Arab population in Palestine. Beatrice Erskine's *Palestine of the Arabs* (1935) and Frances E. Newton's *Fifty Years in Palestine* (1948) are inadequate histories, but they do represent a pro-Palestinian Arab viewpoint. M. F. Abcarius's *Palestine through the Fog of Propaganda* (1946, 1976) is a creditable rendition of how British policy disfavored the Arabs. Similarly, W. F. Boustany's *Palestine Mandate, Invalid and Impracticable* (1936) is a legal account of how the British violated their promises to the Arabs of Palestine. Palestinian Arabs who had been misguided or who had failed as leaders of the national movement during the mandate were reluctant to initiate historical autopsies necessitating some discussion of self-incrimination. Nevertheless, there are at least two notable exceptions in which thorough introspection partially rejected apologia: Musa al-'Alami's *Ibrat Filastin* (The Lesson of Palestine) (1949) and Constantine Zurayq's *Ma'na al-Nakbah* (The Meaning of the Disaster) (1948).⁶ Though not a Palestinian, Zurayq bluntly reminds his readership to accept responsibility for the shortcomings in the Arab world—to resist blaming external factors and to see indigenous

⁶ Musa al-'Alami's *Lesson of Palestine* appeared in abbreviated form in *Middle East Journal* (October 1949): 372–405; Qustantin Zurayq's *Meaning of the Disaster* was ably translated into English by R. Bayly Winder (Beirut, 1956).

weaknesses, defects, and corruptions. His suggested solution for confronting Zionism is to achieve secular nationalism and then attain Arab unity.

Most Arab historians viewed Palestine as a geographic adjunct to greater Syria and Palestinians as a small but integral portion of a larger Arab nation. Because of its significance for the political affairs of the Ottoman empire and for European powers, the Holy Land was a constant source of interest and research for European historians and casual travelers. Unquestionably, the development of Jewish nationalism in the form of Zionism increased motivation to know more about the geography and population of the Holy Land. Investigative and detailed studies, especially of social and economic issues, appeared in German and French prior to 1948. While some are marred by the use of haphazard statistics, each is a substantial and serious work. The works of Hubert Auhagen, Alfred Bonne, Vital Cuinet, Carl Franz Endres, Hans Fischer, Andre Latron, Arthur Ruppin, Leon Schulman, and Jacques Weulersse collectively illuminate the socioeconomic setting for the political struggle that later unfolded between Arab and Jew in Palestine under the mandate.⁷

Many, possibly hundreds, of personal travel accounts and diaries were published by pilgrims or travelers who visited Palestine as part of their journeys to the Middle East in the late nineteenth or early twentieth centuries. Several are particularly noteworthy. After living in Ramallah (north of Jerusalem) for three years, Elihu Grant wrote *The Peasantry of Palestine: The Life, Manners, and Customs of the Village* (1907), a vividly accurate portrait of rural life in Palestine. The classic impressionistic accounts of the traveler to the Holy Land are provided in Laurence Oliphant's two works, *The Land of Gilead* (1881) and *Haifa; or, Life in Modern Palestine* (1887).

Perhaps the most outstanding reference guide and informative overview of Palestine in this period was Harry Luke and Edward Keith-Roach's *Handbook of Palestine and Transjordan* (1934). Both authors served in high positions in Britain's Palestine bureaucracy. Their handbook is a meticulously collected and incisively organized survey of culture, demography, history, and geography. The most reasonably dispassionate summary of mandate affairs emanating from official or semi-official sources in this period was *Great Britain and Palestine, 1915-1945* (Information Paper No. 20, 1946), issued by the Royal Institute of International Affairs. This important volume encapsulates the contentious issues separating Arabs and Jews in Palestine. Two landmark reports are especially worthwhile for their concise review of the mandate and Palestine affairs in general: *The Palestine Royal (Peel) Commission Report* (Cmd. 5479, 1937) and its two volumes of appended testimony, and the *Report of the Anglo-American Committee of Inquiry* (Cmd. 6808, 1946) and its recorded public hearings. Written without present-day archival

⁷ See Hubert Auhagen, *Beiträge zur Kenntnis der Landesnatur und der Landwirtschaft Syriens* (Berlin, 1907); Alfred Bonne, *Palästina, Land und Wirtschaft* (Berlin, 1935); Vital Cuinet, *Syrie, Liban et Palestine: Géographie administrative, statistique, descriptive et raisonnée* (Paris, 1896); Carl Franz Endres, *Die wirtschaftliche Bedeutung Palästinas als Teiles der Türkei* (Berlin, 1918); Hans Fischer, *Wirtschaftsgeographie von Syrien* (Leipzig, 1919); Andre Latron, *La Vie rurale en Syrie et au Liban* (Beirut, 1936); Arthur Ruppin, *Syrien als Wirtschaftsgebiet* (Berlin, 1917); Leon Schulman, *Zur türkischen Agrarfrage, Palästina und die Fellachenwirtschaft* (Weimar, 1916); Jacques Weulersse, *Paysans de Syrie et du Proche-Orient* (Paris, 1946).

material and personal papers, both reports are nevertheless comprehensive and objective accounts of politics and society under the mandate. Personal records of the committee's works are presented by two of its members, R. H. S. Crossman in *Palestine Mission: A Personal Record* (1947) and Bartley C. Crum in *Behind the Silken Curtain: A Personal Account of Anglo-American Diplomacy in Palestine and the Middle East* (1947). A leader of the left wing of the Labor party, Crossman presents a more balanced account than Crum. A California corporation lawyer, Crum divulges the duplicity in America's handling of Arab and Zionist interlocutors, while not hiding his enthusiastic advocacy for Zionism.

Two very fine firsthand accounts of the United Nations' role in the Palestine question are by Jorge Garcia-Granados, *The Birth of Israel: The Drama as I Saw It* (1949), and Herbert V. Evatt, *The Task of Nations* (1949, 1972). Garcia-Granados was a member of the U.N. Special Committee on Palestine, which recommended the partition of Palestine into Arab and Jewish states, and Evatt was president of the U.N. General Assembly in late 1948 during the final stages of Israel's independence war.

Some of the most outstanding early authors and their works on Palestine during the mandate were J. C. Hurewitz's *Struggle for Palestine* (1950, 1976), Ya'acov Shimoni's *'Arave Eretz Yisrael* (The Arabs of Palestine) (1947), and Joseph Vaschitz's *Ha'aravim Be-Eretz Yisrael* (The Arabs in Palestine) (1947). Hurewitz's book is a microscopic study and has remained the best scholarly work on the period from 1936 to 1948. Shimoni's book is prodigious by any standard, presenting an evenly balanced account of Palestinian Arab society and politics during the mandate. In addition, a critically valuable source is the wealth of excellent periodical literature written about the British mandate by some of the important participants and keen observers of the unfolding drama. Representative examples of articles that appeared in contemporaneous periodicals were authored by Auni Abdul Hadi, Chaim Arlosoroff, Omar el-Barghuti, Humphrey Bowman, Alan Cunningham, H. A. R. Gibb, J. C. Hurewitz, O. I. Janowsky, Hubert Montribloux, Fakri al-Nashashibi, Pittman Potter, Salman Rubaschow, Helene Cohn Thon, and B. D. Weinryb.⁸

⁸ See Auni Abdul Hadi, "The Balfour Declaration," *Annals of the American Academy of Political and Social Science*, 164 (November 1932): 14–21; Chaim Arlosoroff, "The Economic Background of the Arab Problem," *Menorah Journal*, 18 (April 1930): 331–45; Omar el-Barghuti, "Judicial Courts among the Bedouin of Palestine," *Journal of the Palestine Oriental Society*, 2 (January 1922): 34–65; Humphrey Bowman, "Some Aspects of Rural Education in Palestine," *Asiatic Review*, 36 (October 1940): 773–79; Alan Cunningham, "Palestine—Last Days of the Mandate," *International Affairs* (October 1948): 481–90; H. A. R. Gibb, "The Islamic Congress at Jerusalem in December 1931," *Survey of International Affairs* (1934): 99–109; J. C. Hurewitz, "Arab Politics in Palestine," *Contemporary Jewish Record*, 5 (December 1942): 597–617; O. I. Janowsky, "Zionism Today," *Menorah Journal* (October 1943): 227–59; Hubert Montribloux, "Palestine 1938: Facteurs économiques et sociaux de conflit Judéo-Arabé," *Sciences politiques*, 54 (April 1939): 170–93; Fakri Nashashibi, "The Arab Position in Palestine," *Royal Central Asian Society Journal*, 23 (January 1936): 16–26; Pittman Potter, "Origin of the System of Mandates under the League of Nations," *American Political Science Review* (November 1922): 563–83; Salman Rubaschow, "Die privatwirtschaftliche Kolonisation in Palästina," *Der Jude* (1921–22): 147–63; Helene Cohn Thon, "Die jüdische Frau in Palästina," *Der Jude* (1920–21): 317–27; B. D. Weinryb, "Socio-economic Relations of Arabs and Jews," *Contemporary Jewish Record* (August 1944): 357–84.

WITH THE INEVITABLE ADVANCE OF PERSPECTIVE, histories written in the post-1950 period generally probe deeper into the complexities of personalities, amplify the causation of events, and systematically investigate political policies that shaped aspects of the conflict's origins. There was a rapid growth of well-researched monographs about the Palestinian and Zionist communities, their interaction, and British policies during the mandate. A unique Palestinian and Israeli historiography emerged as well. Topics investigated included the changing nature of the Ottoman empire and its influence on the conflict's development, Arab-Jewish-British associations over time, Zionism in a European and Palestinian context, Europe's role in framing the competition between Zionism and Arab nationalism, and Arab-Zionist relations during the waning years of the Ottoman empire and during the British presence in Palestine. Most noticeably, in this post-1950 period a distinct Palestinian component evolved in the conflict's historiography. The development of Zionism and the creation of Israel gave Palestinians an unwelcome but shared historical experience. This congruity helped crystallize their identity, leading to a unique set of scholarly inquiries into the composition and politics of the Palestinians. A historiography of the Palestinian community was shaped by its episodic political upheaval and enforced social change. Histories written after 1950 about Palestine and Palestinians reveal two central themes: the precarious and vacillating relationship between Palestinians and other Arabs over the last hundred years, and the emerging definition of Palestinian nationalism in quest of political identity and acceptance.⁹

Especially after the June 1967 War, inquiry intensified about Palestinian aspects of the conflict, paralleling the reemergence of Palestinian nationalism and the quest for a negotiated settlement between Israel and the Arab states. The decades after the June 1967 War emphasized that the Arab-Israeli conflict was more than a state-to-state confrontation, it also contained an important Palestinian dimension. In the 1980s, scholarly attention to the Palestinian component of the conflict was prompted by diplomatic efforts to reconcile Palestinian and Israeli political positions and by international attention to Palestinians living under Israeli control in the West Bank and Gaza Strip.

The 1970s and 1980s yielded the first generation of scholarly works with a historical perspective enhanced by access to previously unavailable primary source materials. Of particular value was newly declassified British documentation from Colonial Office and Foreign Office files, as well as personal papers of British officials who had served during the mandate. Research options were further increased through memoirs published by Arab and Zionist political leaders and through oral interviews with the septuagenarians who had served during the mandate. Because Israeli archivists adopted an increasingly liberal approach to the use of source material (at the Israel State Archives, Central Zionist Archives, and other important smaller archives in Israel), researchers were able to see original documents about the foundation of Zionism, the composition of the Arab community in Palestine, the establishment of Israel, and Britain's

⁹ Continuous indexing of most journal articles on the Palestinians and related topics is available in the "Bibliography of Periodical Literature" section of each issue of the *Middle East Journal* and the *Journal of Palestine Studies*.

changing policies during the mandate. Unfortunately, only relatively few scholars used the newly available English materials in conjunction with the rich lode of previously published Arabic and Hebrew primary source materials, especially memoirs and personal papers.¹⁰

In the 1980s, access to important Palestinian documentation was impeded by the turmoil in Lebanon. The archives of the Institute for Palestine Studies were dispersed after Israel's invasion in June 1982. Established in Lebanon in 1963, the institute had generated many documentary compilations and useful books, such as Abd al-Wahhab al-Kayyali's *Watha'iq al-Muqawama al-Filastiniyyah al-'Arabiyyah didd al-Ithtilal al-Baritani wa al-Sahyuniyyah 1918–1939* (Documents of Palestinian Arab Resistance against British and Zionist Occupation 1918–1939) (1968) and Adnan Abu-Ghazaleh's *Arab Cultural Nationalism in Palestine during the British Mandate* (1973). Coincidentally, in the late 1970s, when Washington took a more active interest in fostering an Arab-Israeli peace, American foreign policy documents were released for general use. Similarly, Israeli foreign policy documents related to the end of the mandate were published in the early 1980s. Inquiry into American interests and policy in the later years of the Palestine mandate began appearing in the late 1970s as initial chapters in books and in articles that focused on the history of American foreign policy toward the Middle East. A spate of new journals appeared from 1970 onward in which scholarly efforts at understanding the conflict's origins were frequently published.¹¹

In the 1960s, historians viewed the emotionally laden topic of the Holocaust as a segment of Jewish history, often separated from the influence it had on the world community's support for the establishment of Israel. Since Israel was a reality, studies began emphasizing aspects of the Jewish community's experience in Palestine—its institutions, personalities, and philosophical divisions before the establishment of the state. There was a proliferation of monographs, including general histories, biographies, and studies about the Jewish community's composition and connection to *Eretz Yisrael*. How, where, and when did an immigrating and pluralistic Jewish community form itself into a social amalgam? How did it interact with the Arabs in Palestine and with the British? Within a decade after the end of the June 1967 War, scores of books were published in Hebrew that reflected a renewed interest in Israel's establishment and the processes that permitted Jewish nation building. Slowly but perceptibly, these histories formed the nucleus for a geographically distinct "Israeli historiography." It emerged from the Jewish and Zionist experience in Palestine, separate from the European origins of Zionism. Notable examples in this genre of scholarship are works by Israeli historical geographers, economic historians, political scientists, and sociol-

¹⁰ Under-utilized memoirs and personal papers include those mentioned in note 5 and those of Eliahu Eliat, Khayriyyah Qasimiyyah, Ahmed Shuqayri, Yosef Weitz, Chaim Weizmann, and Akram Zu'aytar.

¹¹ Important short essays were presented in *Asian and African Studies* (Jerusalem/Haifa), *Ha-mizrah He-hadash* (Jerusalem), and *Middle Eastern Studies* (London), all of which existed before the June 1967 War. A significant or major portion of the contents of journals begun after the war focused on the conflict's origins: *Hatziyonut* (1970-), *Journal of Palestine Studies* (1971-), *Shu'un Filastiniyyah* (1971-), *Cathedra* (1976-), and *Studies in Zionism* (1980-).

ogists: Yehoshua Ben-Arieh, Ruth Kark, Jacob Metzger, Dan Horowitz, and Moshe Lissak.¹²

Only one monograph published in this period superbly covers the historical origins of the conflict from late Ottoman times until World War I. Originally an Oxford doctoral thesis, Neville J. Mandel's *Arabs and Zionism before World War I* (1976) is preeminent because of its fine scholarship. Collected essays in edited volumes provide an excellent source for specific topics on the origins of the conflict. Two volumes were results of international conferences held in Israel during the late 1970s. Moshe Ma'oz's *Studies on Palestine during the Ottoman Period* (1975) and David Kushner's *Palestine in the Late Ottoman Period: Political, Social, and Economic Transformation* (1986) contain important articles on all aspects of Arab and Jewish communal activity in Palestine, primarily during the late nineteenth and early twentieth centuries. Ma'oz's book includes a useful section of essays on archival resources for the history of Ottoman Palestine. Exceptional for its crisp and clear analysis in Ma'oz's book is Israel Kolatt's contribution, "The Organization of the Jewish Population of Palestine and the Development of Its Political Consciousness before World War I." Elie Kedourie and Sylvia G. Haim have edited two collections of fine essays: *Zionism and Arabism in Palestine and Israel* (1982) and *Palestine and Israel in the 19th and 20th Centuries* (1982).

Zionist histories of the Arab-Israeli conflict written after 1950 delve into the heterogeneous origins of Zionism and the multiplicity of methods used by those whose objective was to return the Jews from exile. The goal of national sovereignty was only one of many means advocated for preserving Jewish identity. Five books stand out for defining and explaining the varieties of Zionist thinking: Ben Halpern's *Idea of the Jewish State* (1961, 1969), Arthur Hertzberg's edition of *The Zionist Idea: A Historical Analysis and Reader* (1959), Walter Laqueur's *History of Zionism* (1972), and Shlomo Avineri's *Making of Modern Zionism: The Intellectual Origins of the Jewish State* (1981). At the time of its publication, Halpern's book was pathbreaking for its insights and erudition; Hertzberg's has remained a superb anthology of writings representative of the diversity among Zionist ideologies; Laqueur's tome of more than 600 pages is a thoughtful and illuminating study effectively connecting Zionism's European origins with its evolution in Palestine; and Avineri's work is distinguished for analyzing how eighteen major contributors to Zionist ideology were influenced by their political, social, and religious milieus. The best and most detailed work on Zionism's early development is David Vital's trilogy: *The Origins of Zionism from 1881 to 1897* (1975), *Zionism: The Formative Years from 1897 to 1907* (1982), and *Zionism: The Crucial Phase from 1907 to 1920* (1987).

Although biography as a genre of Zionist historiography already existed in the pre-1950 period, it was not until the 1980s that several outstanding biographies of Zionist leaders were published. They include Anita Shapira's *Berl: The Biography*

¹² See Yehoshua Ben-Arieh, *Jerusalem in the 19th Century: The Old City* (Jerusalem, 1984); Evyatar Friesel, *Hamediniyut Hatzionit Le-ahar Hatzharat Balfur 1917–1922* [Zionist Policy after the Balfour Declaration 1917–1922] (Tel Aviv, 1977); Ruth Kark, ed., *The Land That Became Israel: Studies in Historical Geography* (Jerusalem, 1989); Jacob Metzger, *Hon Leumi Le-bayit Leumi: 1919–1921* [National Capital for a National Home] (Jerusalem, 1979); Dan Horowitz and Moshe Lissak, *Origins of the Israeli Polity*, Charles Hoffman, trans. (Chicago, 1978).

of a Socialist Zionist, Berl Katznelson, 1887–1944 (1984), Shabtai Teveth's *Ben-Gurion: The Burning Ground 1886–1948* (1987), Jehuda Reinharz's *Chaim Weizmann: The Making of a Zionist Leader* (1985), and Amos Perlmutter's *Life and Times of Menachem Begin* (1987).

While understanding that Palestine had Arab inhabitants, the earliest European Zionist writers tended to minimize the confrontation that would ensue. In contrast, the earliest Zionist leaders of the Jewish community in Palestine comprehend and describe the existing and escalating tension with the Arabs. Three historians have made important contributions to the understanding of Arab-Jewish relations during the mandate: Neil Caplan in *Palestine Jewry and the Arab Question, 1917–1925* (1978) and the two volumes of *Futile Diplomacy* (1983), Yosef Gorny in *Zionism and the Arabs 1882–1948* (1987), and Shabtai Teveth in *Ben-Gurion and the Palestinian Arabs: From Peace to War* (1985). Collectively, the theses in these books demonstrate that Zionist understandings of and strategies toward the “Arab Problem” were sophisticated, analytical, pragmatic, and constantly subject to revision. Superseding all Zionist policy options and attitudes toward the Arabs, however, was the *a priori* and consummate dedication to establishing the Jewish national home. During this historiographic period, an extensive array of publications appeared that focus less on ideology and more on the physical, demographic, and institutional growth of the Jewish community in Palestine. From a wide variety of political, social, geographic, and economic perspectives, a series of English and Hebrew works (including books, articles, and masters and doctoral theses) painstakingly investigate the Jewish presence and expansion during the Ottoman and mandate periods. A representative sampling of this rich historical category is derived from many publications, including those of the Israeli-based *Yad Ben Zvi* and *Yad Tebenkin* institutes.¹³

In the academic controversy about the conflict's origins, numerous studies have analyzed the right and privilege to control the geographic area of Palestine in the post-World War I period. The central issue is whether the area of Palestine was or was not to be excluded in a proposed independent Arab state at the conclusion of World War I. The historical debate is based on what was said, what was meant, what was perceived, and who had the right to make commitments. A wide range of explications has been given to the various declarations, agreements, correspondences, memoranda, treaties, statements, and commission findings that emerged during and immediately after World War I about Palestine's future. Multiple scholarly interpretations of this key issue were published in the post-1950 period and may be found in the works of Isaiah Friedman, Elie Kedourie, A. L. Tibawi, and Zeine Zeine.¹⁴ Ronald Sanders, in *The High Walls of Jerusalem: A History of the*

¹³ See, for example, Arie Bitan, *Temurot Yishuviot Ba-galil Ha-tahton Ha-mizrahi (1800–1978)* [Changes of Settlement in the Eastern Lower Galilee (1800–1978)] (Jerusalem, 1982); Shmuel Dothan, *Pulmus Ha-halukah Bi-tekufat Ha-mandat* [The Partition Controversy in the Mandatory Period] (Jerusalem, 1979); Yehoshua Kaniel, *Hemshekh Utemurah: Ha-yishuv Ha-yashan Veba-yishuv Ha-hadash bi-tekufat Ha'aliyah Ha-rishonah Veba-sheniyah* [Continuity and Change: Old Yishuv and New Yishuv during the First and Second Aliyah] (Jerusalem, 1981); Simon Schama, *Two Rothschilds and the Land of Israel* (New York, 1978); and Zvi Shiloni, *Ha-keren Ha-kayemet Le-yisrael Veba-hityashvut Ha-tziyonit 1903–1914* [The Jewish National Fund and Zionist Settlement 1903–1914] (Jerusalem, 1990).

¹⁴ See Isaiah Friedman, *The Question of Palestine, 1914–1918* (New York, 1973); Elie Kedourie, *In*

Balfour Declaration and the Birth of the British Mandate for Palestine (1983), offers a long but excellent synthesis of the period's diplomatic history. David Fromkin's *Peace to End All Peace: The Fall of the Ottoman Empire and the Creation of the Modern Middle East* (1990) is the best popular narrative of this critical period.

The most readable short history of the mandate period is Christopher Sykes's *Crossroads to Israel, 1917–1948* (1973). In spite of a latent pro-Zionist bias, it is an excellent synopsis of British, Jewish, and Palestinian interactions. Epitomizing meritorious scholarship on the origins of the conflict are Yehoushua Porath's two volumes: *The Emergence of the Palestinian-Arab National Movement, 1918–1929* (1974) and *The Palestinian Arab National Movement: From Riots to Rebellion, 1929–1939* (1977). My book, *The Land Question in Palestine, 1917–1939* (1984), addresses the social and economic processes of change that adversely affected the majority Arab rural population in its confrontation with Zionist development. Documentary evidence suggested three reasons why Zionist physical growth began and remained essentially unchallenged until the late 1930s: significant numbers of Arab peasants and notables sold portions of their patrimony to Jewish immigrants, the majority rural Arab population's desperate economic condition depleted its interest and ability to compete with Zionism's physical growth, and the British did not fulfill their pledge of financial and paternalistic support to the Arab population.

Solid histories of the Palestinian Arab community's political, social, organizational, and economic characteristics can be found in works by Yuval Arnon-Ohanah, Michael Asaf, Ann Lesch, Joel S. Migdal, Uri M. Kupferschmidt, Taysir N. Nashif, and Roger Owen.¹⁵ Two books by Jon Kimche and David Kimche present cogent assessments of illegal Jewish immigration to Palestine and the Israeli independence war: *The Secret Roads: The "Illegal" Migration of a People, 1938–1948* (1955, 1976) and *Both Sides of the Hill: Britain and the Palestine War* (1960).

The British role in the evolution of the mandate and Arab-Jewish relations has been treated extensively in quantity and quality. Superior examples are Michael J. Cohen's two works, *Palestine and the Great Powers, 1945–1948* (1982) and *Palestine, Retreat from the Mandate: The Making of British Policy, 1936–45* (1978); Norman A. Rose's *Gentile Zionists: A Study in Anglo-Zionist Diplomacy, 1929–1939* (1973); Bernard Wasserstein's distinguished *The British in Palestine* (second edition, 1990);

the Anglo-Arab Labyrinth: The McMahon-Husayn Correspondence and Its Interpretations, 1914–1939 (Cambridge, 1976); A. L. Tibawi, *Anglo-Arab Relations and the Question of Palestine, 1914–1921*, 2d edn. (London, 1978); Zeine N. Zeine, *The Struggle for Arab Independence: Western Diplomacy and the Rise and Fall of Faisal's Kingdom in Syria* (Beirut, 1960).

¹⁵ See Yuval Arnon-Ohanah, *Falahim Ba-mered Ha-'aravi be-Eretz Yisrael, 1936–1939* [Fellaheen in the Palestine Revolt, 1936–1939] (Tel Aviv, 1978); Michael Asaf's pioneering work on the Arab community, *Hit'orrut Ha-'aravim Veyehudim Be-Eretz Yisrael* [The Arab Awakening in Palestine and Their Flight] (Tel Aviv, 1967); Ann Mosely Lesch, *Arab Politics in Palestine, 1917–1939: The Frustration of a Nationalist Movement* (Ithaca, N.Y., 1979); Joel S. Migdal, ed., *Palestinian Society and Politics* (Princeton, N.J., 1980); Uri M. Kupferschmidt, *The Supreme Muslim Council: Islam under the British Mandate for Palestine* (Leiden, 1987); Taysir N. Nashif, *The Palestine Arab and Jewish Political Leaderships: A Comparative Study* (New York, 1979); Roger Owen, ed., *Studies in the Economic and Social History of Palestine in the Nineteenth and Twentieth Centuries* (Oxford, 1982). Kuperschmidt's work is especially noteworthy because it is an in-depth institutional history.

and Ronald W. Zweig's *Britain and Palestine during the Second World War* (1986). Other very good works concentrating on the British presence in Palestine are Ylana N. Miller's *Government and Society in Rural Palestine, 1920–1948* (1985) and the various essays and journal articles by Gabriel Sheffer. All of these authors offer sober analysis of British or Zionist policy based on primary sources from the Public Record Office and Zionist Archives.

Only recently have the last years of the mandate and U.S. interest in the conflict become the focus of scholarly discussion. Of noteworthy attention are Ilan Pappé's *Britain and the Arab-Israeli Conflict, 1948–51* (1988) and two of the seven essays in William Roger Louis and Robert W. Stookey's edition of *The End of the Palestine Mandate* (1986). Louis's own essay focuses on Ernest Bevin's important role as British foreign secretary; Walid Khalidi's fine article discusses the inter-Arab rivalries that plagued the Palestine issue in the 1945–1948 period.

THE THIRD AND MOST RECENT HISTORIOGRAPHICAL PERIOD covers histories written and published from the mid-1980s to the present. This group of histories represents the shift from merely recounting causes to providing nuances, from only describing events to analyzing key aspects of the conflict's beginnings and turning points. Zionism, Israel, and components of the Palestinian community are probed in greater detail than before. The advent of available documentary and archival material from the late 1940s and early 1950s provided the substantive base for a close examination of this singularly most emotional and traumatic period in the conflict's origins. The last years of the mandate, the first years of Israel's establishment, and Israel's relations with surrounding Arab states have become a distinct area of inquiry. A portion of these works, particularly those investigated by Israeli and former Israeli scholars, scrutinizes Zionist policy, the actions of Zionist-Israeli leaders, and explains the components, tone, and severity of animosity that developed between the Arab and Zionist communities.

Two general points are made by the "revisionist" historians: Israeli and Zionist leadership had broader and deeper contacts with their Arab neighbors than historians had previously understood, and some of the Zionist leaders were directly to blame for creating the Palestinian refugee problem. The small number of writers on this topic, led by Benny Morris's *Birth of the Palestinian Refugee Problem, 1947–1949* (1987) and Avi Shlaim's *Collusion across the Jordan: King Abdullah, the Zionist Movement, and the Partition of Palestine* (1988), contribute important additions to earlier research published in Hebrew. Although their conclusions are not new, these authors are the first to present a vivid and detailed description in English of the frequency, methods, and motivations associated with the Arab-Israeli state and inter-Arab relationships. The essence of the recent histories has been who-said-what-to-whom-and-when and who-did-what-to-whom-and-why. The debate about culpability—who was responsible for the Palestinian refugee problem—became the topic of conferences and public debate in Israel. From a scholarly controversy, it evolved into a contentious feud charged with malice, characterized by claims of anti-Israeli/pro-Arab sympathy, and

reduced unnecessarily to spiteful personal attacks.¹⁶ The episodic and largely Israeli academic response to Morris and Shlaim was a result of their more detailed explanations of previous postulations. They provide essential texture and depth to the general understanding of the motivation of individuals who influenced events during this most sensitive period in the conflict's origins.

Shlaim's main thesis is that Emir Abdullah of Transjordan and the Zionist leadership in Palestine reached an understanding about sectioning Palestine after the mandate's conclusion, with Britain as a willing conspirator. Abdullah's furtive and intermittent relationship with Zionists originated in the early 1930s and outlasted the end of the mandate. While it was known that to varying degrees Arabs, Jews, and the context of the war were all responsible for the creation of the Palestinian refugee problem, Morris was the first to annotate and chronicle its emergence in great detail. Although neither Shlaim nor Morris amplify an old cause or discover a new one in recounting the origins of the conflict, they contribute to its historiography by clarifying the processes of human interaction in the Zionist-Arab struggle to control Palestine. A few works less well known than those of Shlaim and Morris do add substantive dimensions and details to the period. Their authors, Dan Schueftan, Tom Segev, Avraham Sela, and Mary Wilson, concur that Jordan's Emir Abdullah succeeded in making a deal with the Zionists to take a portion of Palestine's territory west of the Jordan River.¹⁷ There is less agreement about whether the arrangement was merely a tacit understanding revised as the 1948 fighting progressed or was explicit. The debate about the promises between Abdullah and the Zionists in the 1947–1950 period is reminiscent of the unclosed historical dispute about pledges and perceptions among Arabs and Zionists during the World War I period.

Two decades before Morris and Shlaim published their "revisionist" histories, Israeli historians had initiated a scholarly review of Arab-Jewish and Arab-Zionist contacts extending from the 1880s to the late 1940s. In his 1965 publications in *Middle Eastern Studies* and *St. Antony's Papers*, Neville Mandel recounted how the Zionists circumvented Ottoman restrictions against Jewish growth in Palestine. His research revealed two new and important points. First, Arab antagonism for the early immigrating Zionists was pronounced, prominent, and widespread in Palestine prior to World War I; second, the Balfour Declaration of 1917 was not so much the starting point of the conflict as a turning point that greatly aggravated an existing trend of animosity between the two communities. In 1968, Ya'acov Ro'i's article in *Middle Eastern Studies* incisively depicted Zionist attitudes

¹⁶ A verbal war has developed between Shabtai Teveth, David Ben-Gurion's biographer, and Benny Morris, the author of *The Birth of the Palestinian Refugee Problem*; see the Israeli daily *Ha'aretz* of April 7, 14, and 21, May 9 and 19, 1989; *Tikkun* (November–December 1988); Benny Morris, "The New Historiography," in his *1948 and After: Israel and the Palestinians* (Oxford, 1990), 1–34; Shabtai Teveth, "Charging Israel with Original Sin," *Commentary* (September 1989): 24–33; and "The Palestine Refugee Problem and Its Origins," *Middle Eastern Studies* (April 1990): 214–49.

¹⁷ See Dan Schueftan, *Optziyah Yardenit* [A Jordanian Option] (Tel Aviv, 1986); Tom Segev, *1949: Ha-yisre'elim Ha-rishonim* [1949: The First Israelis] (Jerusalem, 1984); Avraham Sela, *Mi-maga'im Le-ma'sa-U-matan: Yahase Ha-sokhnut Ha-yehudit U-medinat Yisrael 'Im Ha-melekh 'Abdullah, 1946–1950* [From Contacts to Negotiation—Jewish Agency and Israel State Relations with King Abdullah, 1946–1950] (Tel Aviv, 1985); Mary Wilson, *King Abdullah, Britain and the Making of Jordan* (Cambridge, 1988).

toward the Arabs before World War I and made it clear that the Zionists knew they had a serious problem with the indigenous Arab population. Ro'i's work is based on an unpublished master's thesis completed at the Hebrew University of Jerusalem in 1964. In 1970, Meir Verete published an article on the origins of the Balfour Declaration in *Middle Eastern Studies*. He discredited the widely held notion that Britain's promise in 1917 to the Jews to facilitate the establishment of a national home was motivated by blind emotional pathos for Zionism. Yosef Luntz's Hebrew article in *Ha-mizrah He-hadash* in 1972, "Diplomatic Contacts between the Zionist Movement and the Arab National Movement at the Close of the First World War," gives a fascinating analysis of the meetings of Zionist leaders with Arab counterparts in Damascus and Constantinople. Morris and Shlaim benefited from the earlier works of these Israeli historians who had chronicled Arab-Jewish contacts and described emerging and changing Zionist policy toward the Arab community in Palestine.

In a study that continues the trail of Jewish or Zionist-Arab contacts, Itamar Rabinovich, *The Road Not Taken: Early Arab-Israeli Negotiations* (1991), discusses the Arab-Israeli conflict's formative years in the wake of the 1948 war and armistice agreements signed in 1949. Rabinovich contends that Israel's quest for security and the Arab quest for vindication cannot be reconciled; finding a solution to Arab-Israeli differences is impeded by the issues of territory and refugees. Three other works focus on the Palestinian Arab society and politics. Issa Khalaf's *Politics in Palestine: Arab Factionalism and Social Disintegration, 1939-1948* (1991) is an excellent in-depth study of the political disorientation of the Palestinian community and its leadership in the 1940s in Palestine. Khalaf's treatment of the Palestinian Arab leadership, particularly Hajj Amin al-Husayni, is distinctly harsher than Philip Mattar's in *The Mufti of Jerusalem: Al-Hajj Amin al-Husayni and the Palestinian National Movement* (1988). Mattar presents a more sympathetic assessment of the Mufti's key role in shaping the Palestinian Arab response to Zionism and the British mandate. Perhaps the best collection of essays published in this period was Gad G. Gilbar's *Ottoman Palestine, 1800-1914: Studies in Economic and Social History* (1990). The insightful use of Ottoman source material in each article is admirable.

ALTHOUGH THERE IS NO DEARTH OF SOURCE MATERIAL on the origins of the Arab-Israeli conflict, there is a shortage of trained Middle Eastern historians who read Arabic and especially Hebrew and who divorce themselves from contemporary politics when writing about the conflict's origins. Because of political outlook, impatience, or a penchant for brevity, some authors do not succeed in putting terms and concepts (such as state, nation, and independence) into historical perspective. Advocacy of a political viewpoint may supersede nuances of terminology, the causation of events, or the identification of mechanisms of change in the conflict's evolution. Issues that were controversial ten, fifty, or eighty years ago remain contentious today. During World War I, did the British include Palestine in the promise for Arab independence? Cursory essays or alleged histories of the

Arab-Israeli conflict often function as if the time since World War I has negated the significance of this critical question. Scholarship is not the relegation of a historical controversy to a simplified and polemical assertion of the author's preference.¹⁸

Until very recently, a sense of proportion was absent from the writings on the conflict's origins. During the late Ottoman and mandate periods, Zionists and Arabs did not absolutely detest one another. They were people with differing levels of fears, aspirations, and commitments to their communities, people being influenced by a variety of external forces. Although a review of the 1947–1950 period reveals an amplification of previous assumptions, a study of the social, cultural, and economic dimensions of the entire period until 1950 has only recently begun.¹⁹ Now that political histories are being written about the leaders of the Zionist and Palestinian communities, it is necessary to refine the distinctions about the nature and composition of the common people who participated in the conflict's origins.

Finally, in all three historiographic periods, Jewish, Zionist, or Israeli authors have dominated investigation of the politics of the conflict. Motivations for studying the causes of the conflict have not changed greatly: the passion of Jews throughout the world to know more about their common origins, a need to understand Israel's creation and sustenance in the shadow of the Holocaust, and the Palestinian Arab community's interest in comprehending its relationship to Zionism in the past and to Israelis in the present. Since 1948, the Palestinians have waged a prolonged battle for recognition and identity. Once the Palestinians and other Arab states find a solution to their present differences with Israel, another historiographic period may be generated for further study about the beginnings of the Arab-Israeli conflict. Perhaps then, the emphasis of many Middle Eastern historians will be transposed from political interpretations of the conflict's current status to a fuller exploration of its origins.

¹⁸ See Deborah J. Gerner, *One Land, Two Peoples: The Conflict over Palestine* (Boulder, Colo., 1991), 29; and Tareq Y. Ismael and Jacqueline S. Ismael, "Palestine and the Palestine Question," in their edited volume, *Politics and Government in the Middle East and North Africa* (Miami, Fla., 1991), 293.

¹⁹ Examples of initial research in the social, economic, and cultural fields of the mandate and its populations can be found in Rachelle Taquq, "Peasants into Workmen: Internal Labor Migration and the Arab Village Community under the Mandate," in Migdal, *Palestinian Society and Politics*, 261–86; Jacob Metzer and Oded Kaplan, *Meshek Yehudi Ve-meshek 'Aravi Be-Eretz Yisrael Tozar Ta'asukah Ve-zemitha Betkufat Ha-mandat* [The Jewish and Arab Economies in Eretz Yisrael: Product, Employment and Growth in the Mandate Period] (Jerusalem, 1990); Kenneth W. Stein, "Palestine's Rural Economy, 1917–1939," *Studies in Zionism*, 8 (Spring 1987): 25–49; A. L. Tibawi, "English Education for Palestine Arabs 1914–1930," *Orient*, 23 (March 1982): 106–21.

Themes in the History of the State of Israel

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THE STATE OF ISRAEL declared its independence on May 14, 1948, effective with the termination of the British mandate of Palestine on May 15, but its creation was preceded by more than fifty years of efforts by the Zionist movement to establish an independent Jewish state in Palestine as a solution to the Jewish question.¹

Although Israel is a new state, it is located in the historic area where ancient Jewish kingdoms had existed, and it has a long and complex background.² The history of the modern Jewish state antedates its independence and may be traced to the nineteenth century, when secular Jewish immigration to Palestine grew and when the practical, modern effort to establish a state began with the development of political Zionism and the creation of the World Zionist Organization by Theodor Herzl.³ The process gained impetus with the issuance of the Balfour Declaration in November 1917, in which the British government expressed support for the establishment of a Jewish national home in Palestine. Jewish immigration to Palestine continued throughout the following years, but, with the advent of the Nazi regime in Germany in the 1930s and the Holocaust that followed, the need and the numbers grew rapidly. With the end of World War II,

¹ Contrary to the situation for most states in the Middle East, the literature on Israel is vast. Almost all aspects of Israel have been the subject of extensive writings by scholars, journalists, participants, and observers. In addition, there is the voluminous literature concerning the Arab-Israeli conflict. The abundance of material both facilitates and hinders the study, research, and analysis of Israeli history, as the reader is faced with a bewildering choice. For general histories of Israel that begin before independence, see Howard M. Sachar, *A History of Israel: From the Rise of Zionism to Our Time* (New York, 1976); and Noah Lucas, *The Modern History of Israel* (New York, 1975). See also Abba Eban, *My Country: The Story of Modern Israel* (New York, 1972). A unique perspective is provided by David Ben-Gurion, *Israel: A Personal History*, Nechemia Meyers and Uzy Nystar, trans. (New York, 1971). On Zionism, Shlomo Avineri, *The Making of Modern Zionism: The Intellectual Origins of the Jewish State* (New York, 1981); and Walter Laqueur, *A History of Zionism* (New York, 1972), provide a comprehensive history and analysis of the movement. Ben Halpern, *The Idea of the Jewish State*, 2d edn. (Cambridge, Mass., 1969), focuses on the origins and development of the Zionist idea. See also Arthur Hertzberg, ed., *The Zionist Idea: A Historical Analysis and Reader* (New York, 1959); and the trilogy by David Vital, *The Origins of Zionism* (London, 1975), *Zionism: The Formative Years* (New York, 1982), and *Zionism: The Crucial Phase* (New York, 1987).

² In a real sense, Jewish history forms the pre-history of the modern state of Israel. Understanding Israel requires inquiry into Jewish history as well as into the pre-state period of the British mandate and the Zionist efforts at state creation. For an introduction to the subject, see David Ben-Gurion, *The Jews in Their Land* (Garden City, N.Y., 1966); Louis Finkelstein, ed., *The Jews: Their History, Culture and Religion* (New York, 1949); Heinrich Graetz, *History of the Jews*, 6 vols. (Philadelphia, 1891–98); and Abba Eban, *My People: The Story of the Jews* (New York, 1969).

³ See Alex Bein, *Theodor Herzl: A Biography* (Philadelphia, 1940); Amos Elon, *Herzl* (New York, 1975); and Theodor Herzl, *Complete Diaries*, 5 vols., Raphael Patai, ed., Harry Zohn, trans. (New York, 1960).

there was strong pressure, especially from President Harry S. Truman, for the remnants of European Jewry to be permitted to immigrate to Palestine despite British restrictions.⁴ The Arab reaction to the influx of Jews into Palestine and to the effort to create a Jewish state there was negative and often violent.

The British decision in 1947 to relinquish the mandate generated months of inquiry and debate, after which the U.N. General Assembly, on November 29, 1947, adopted the majority plan of the U.N. Special Committee on Palestine (UNSCOP) that called for the partition of Palestine into a Jewish state and an Arab state and for an international administration over the city of Jerusalem. Accepted with some reluctance by the Zionists, the plan was denounced by the Arab world leadership, which prepared for war to ensure that all of Palestine would be included in an Arab state.

Upon independence, David Ben-Gurion became prime minister, and Chaim Weizmann was selected as president of Israel.⁵ The new government was soon recognized by the United States and the Soviet Union, as well as by other states. The Arab League declared war, and the armies of the neighboring Arab states entered Palestine to restore the territory to Arab control. The first Arab-Israeli war—known in Israel as the War of Independence⁶—was a long and bitter one between Israel and the armies of Egypt, Jordan, Syria, Lebanon, and Iraq with assistance from other Arab League members.

In the spring of 1949, Israel signed armistice agreements with each of its neighbors (Egypt, Syria, Jordan, and Lebanon) that established the frontiers (that is, armistice lines) that existed until the June 1967 War. But peace negotiations and treaties to establish permanent borders and to resolve other aspects of the Arab-Israeli conflict did not follow. As a consequence of the war, Israel encompassed more territory than had been allocated to it by the partition plan, and portions of the territory allocated to the Palestinian Arab state came under Egyptian and Jordanian control (the Gaza Strip and the West Bank, respectively, including the eastern sector and the old walled city of Jerusalem).

⁴ On Truman's views and role in regard to Jewish refugees as well as in regard to the creation of Israel, see Michael J. Cohen, *Truman and Israel* (Berkeley, Calif., 1990); and Zvi Ganin, *Truman, American Jewry, and Israel, 1945–1948* (New York, 1979).

⁵ See Michael Bar-Zohar, *Ben-Gurion: The Armed Prophet*, Len Ortzen, trans. (Englewood Cliffs, N.J., 1966); Michael Bar-Zohar, *Ben-Gurion: A Biography*, Peretz Kidron, trans. (New York, 1979); David Ben-Gurion, *Rebirth and Destiny of Israel*, Mordechai Nurock, ed. (New York, 1954); Shabtai Teveth, *Ben-Gurion: The Burning Ground, 1886–1948* (Boston, 1987); Shabtai Teveth, *Ben-Gurion and the Palestinian Arabs: From Peace to War* (New York, 1985); Dan Kurzman, *Ben-Gurion: Prophet of Fire* (New York, 1983); and Barnett Litvinoff, *Ben-Gurion of Israel* (New York, 1954); Isaiah Berlin, *Chaim Weizmann* (London, 1958); Jehuda Reinharz, *Chaim Weizmann: The Making of a Zionist Leader* (New York, 1985); Samuel Shihor, *Hollow Glory: The Last Days of Chaim Weizmann, First President of Israel* (New York, 1960); Meyer W. Weisgal and Joel Carmichael, eds., *Chaim Weizmann: A Biography by Several Hands* (New York, 1963); Chaim Weizmann, *Trial and Error: The Autobiography of Chaim Weizmann* (New York, 1949); and Barnett Litvinoff, *Weizmann: Last of the Patriarchs* (New York, 1976).

⁶ See Netanel Lorch, *The Edge of the Sword: Israel's War of Independence, 1947–1949* (New York, 1961); Lynne Reid Banks, *Torn Country: An Oral History of the Israeli War of Independence* (New York, 1982); and Jon Kimche and David Kimche, *A Clash of Destinies: The Arab-Jewish War and the Founding of the State of Israel* (New York, 1960).

ISRAEL IS A PARLIAMENTARY DEMOCRACY without a written constitution, although there are a number of basic laws enacted by the parliament (Knesset) that provide a framework for governmental action.⁷ Israel's political institutions include a president, a government headed by a prime minister with executive power, and a parliament of 120 members elected by the population in a national election held at least every four years. The first election was held in February 1949.

Israel's political dynamics revolve around the political intensity, diversity, and participation of Israelis, the dominance of the political parties, and the central role of the political elite.⁸ Israelis are intensely interested in politics and governmental decision making and participate actively in political life. Often, the traditions and practices of politics derive from the experiences of Israel's immigrant population in their countries of origin, although a distinctly "Israeli" political culture has developed over the decades of the mandatory and post-independence periods. Israelis also hold widely divergent views on all aspects of national life, and this, combined with the characteristics of Israel's proportional representation electoral system, has generated a large number of political parties and extensive competition among them. Partly as a consequence, there has never been a majority party. All of Israel's governments have been coalitions composed of a number of political parties coming together to promote agreed-upon programs arrived at by compromising the views and policies of the participants.

Despite the plethora of parties and the consequent creation of political coalitions to govern, two political parties and their leaders have dominated Israeli political life.⁹ Labor and its constituent or predecessor units on the left and Likud on the right have led all the coalition governments since independence. Both owe their ideological underpinnings and, particularly in the case of Labor, their leadership to pre-independence movements.

The dominance of Israel's political system by the left-of-center Labor party and its predecessors began in the Yishuv (the Jewish community during the mandate) when Mapai, under the leadership of David Ben-Gurion, became the most prominent and important political party and its role went virtually unchallenged.¹⁰ It continued its dominance after independence, obtaining the largest percentage of votes in Knesset elections until 1977. Labor was the controlling member of government coalitions and generally held the important portfolios of prime minister, defense minister, foreign minister, and finance minister. It was also significant in the Jewish Agency (a body created by the League of Nations in the 1920s to represent the Jews of Palestine in international bodies) and in the

⁷ On the parliament, see Asher Zidon, *Knesset: The Parliament of Israel*, Aryeh Rubinstein and Gertrude Hirscher, trans. (1957; New York, 1967); Eliahu S. Likhovski, *Israel's Parliament: The Law of the Knesset* (London, 1971); Samuel Sager, *The Parliamentary System of Israel* (Syracuse, N.Y., 1985); and Gregory S. Mahler, *The Knesset: Parliament in the Israeli Political System* (Rutherford, N.J., 1981).

⁸ On the general nature of Israeli politics and society, see Bernard Reich, *Israel: Land of Tradition and Conflict* (Boulder, Colo., 1985); and Bernard Reich and Gershon R. Kieval, eds., *Israel Faces the Future* (New York, 1986). Nadav Safran, in *Israel: The Embattled Ally* (Cambridge, Mass., 1981), provides coverage of Israel's domestic scene as it affects foreign policy.

⁹ See Lester G. Seligman, *Leadership in a New Nation: Political Development in Israel* (New York, 1964); and Ira Sharkansky, *What Makes Israel Tick? How Domestic Policy-Makers Cope with Constraints* (Chicago, 1985).

¹⁰ See Peter Y. Medding, *Mapai in Israel: Political Organization and Government in a New Society* (Cambridge, 1972).

Histadrut (the labor federation). The party became a political institution and was identified with the state in the minds of many voters. Additionally, because of its long tenure in office, it permeated the governmental bureaucracy as well as economic and other institutions. Labor was the party of the founders of the State of Israel (including such well-known figures as Golda Meir and Moshe Dayan), the party of independence, and the party of the great military victories of the Six-Day War.¹¹

Nevertheless, Labor declined. Numerous reasons have been advanced for the downturn in Labor's fortunes compared to Likud, but they seem to coalesce around lackluster leadership, rivalries and infighting among the salient figures, an inability to achieve broad support among the growing Oriental Jewish political constituency and a concomitant decline in the size of the traditional European-based Labor constituency, and a lack of younger-generation leaders to assure continuity.

Labor controlled Israel's government from independence until 1977, when parliamentary elections, known as the political "earthquake," transferred control to the Likud, and Menachem Begin succeeded Yitzhak Rabin as prime minister.¹² The Likud has been a dominant force in Israeli politics ever since. Although its control of the system continues to expand, it has not yet achieved the pervasiveness of Labor in the pre-1977 period. Begin sought to reorient Israel's domestic and foreign policy in keeping with the Zionist-Revisionist ideology developed decades earlier by Vladimir Zeev Jabotinsky.¹³ Even though there were domestic economic and social changes, the major achievement was the peace treaty with Egypt for which Begin shared a Nobel Peace Prize with Egyptian President Anwar Sadat. Within Israel, despite growing triple-digit inflation in the 1980s, Begin's tenure was marked by prosperity for the average citizen: the standard of living rose. Begin proved to be a commanding figure. When he resigned in September 1983, he was succeeded by Yitzhak Shamir, who followed a policy of continuity and sought to build Likud's constituency.¹⁴

In the parliamentary elections of 1984, neither of the two major parties gained sufficient strength to form a coalition. In September 1984, Shimon Peres¹⁵ and Yitzhak Shamir agreed to the formation of a National Unity Government (NUG)

¹¹ See Marie Syrkin, *Golda Meir: Woman with a Cause* (New York, 1963); Golda Meir, *A Land of Our Own: An Oral Autobiography*, Marie Syrkin, ed. (New York, 1973); and Golda Meir, *My Life* (New York, 1975). Shabtai Teveth, *Moshe Dayan: The Soldier, the Man, the Legend* (Boston, 1973); Naphtali Lau-Lavie, *Moshe Dayan: A Biography* (Hartford, Conn., 1969); and Moshe Dayan, *Story of My Life* (New York, 1976).

¹² See Eric Silver, *Begin: The Haunted Prophet* (New York, 1984); Menachem Begin, *The Revolt* (1951; New York, 1983); Ned Temko, *To Win or to Die: A Personal Portrait of Menachem Begin* (New York, 1987); Amos Perlmutter, *The Life and Times of Menachem Begin* (New York, 1987); Frank Gervasi, *The Life and Times of Menachem Begin: Rebel to Statesman* (New York, 1979); and Eitan Haber, *Menachem Begin: The Legend and the Man*, Louis Williams, trans. (New York, 1978). Yitzhak Rabin, *The Rabin Memoirs* (Boston, 1979).

¹³ See Joseph B. Schechtman, *Rebel and Statesman: The Vladimir Jabotinsky Story*, vol. 1: *The Early Years* (New York, 1956); and Joseph B. Schechtman, *Fighter and Prophet: The Last Years*, vol. 2: *The Vladimir Jabotinsky Story* (New York, 1961).

¹⁴ No major works have been written about Shamir in English. For a brief biography, see Bernard Reich and Joseph Helman, "Yitzhak Shamir," in Bernard Reich, ed., *Political Leaders of the Contemporary Middle East and North Africa: A Biographical Dictionary* (Westport, Conn., 1990), 486–94.

¹⁵ See Matti Golan, *Shimon Peres: A Biography* (London, 1982).

in which they would rotate in the positions of prime minister and foreign minister.¹⁶ The results of the 1988 election again required a National Unity Government, but this time the position of prime minister was retained by Shamir while Peres became finance minister. Bickering within the government and differences over the U.S.-initiated Arab-Israeli peace process led to the dissolution of the coalition and a successful vote of no confidence in the spring of 1990. This formally terminated the Shamir-led National Unity Government and set in motion substantial maneuvering within and among Israel's political parties. In June 1990, Shamir presented a new Likud-led right-of-center government to the Knesset for its approval and thus became prime minister without the burden of sharing power with the left-of-center Labor party and its allies. Subsequent political soundings suggest the likelihood that the combination of Likud and its right-of-center allies will grow in political strength at the expense of Labor and the left-of-center in the decade of the 1990s, as the political constituencies of Israel shift toward the political right and the dominant issues of foreign and security policy retain their centrality for the Israeli voter.¹⁷

THE INGATHERING OF THE EXILES and the building of a new state is one of the central themes of the State of Israel. At independence, Israel had some 806,000 citizens (650,000 Jews and 156,000 non-Jews, often referred to as "Arabs").¹⁸ The non-Jewish population has quintupled since 1948, mostly as a result of high birth rates. By contrast, the Jewish population has increased more than six-fold since independence with more than two million Jewish immigrants.

Israel's Declaration of Independence connects the Jewish people to the Land of Israel (*Eretz Yisrael*), where "their spiritual, religious and political identity was shaped," and notes that "The State of Israel will be open for Jewish immigration and for the Ingathering of the Exiles." To effect this principle, the Knesset adopted the Law of Return on July 5, 1950. It assures virtually unlimited and unfettered Jewish immigration to Israel by providing every Jew with the right to immigrate and settle there unless engaged in an activity "directed against the Jewish people" or one that may endanger public welfare or "public health or the security of the state." The Law of Return has provided the formal basis for the substantial immigration that has taken place since independence.

Despite its small size, warfare with the Arabs, constant tension, and a poor endowment of natural resources, Israel has continued to fulfill its self-proclaimed mission as the haven for oppressed and distressed Jews located elsewhere. This concept, derived from the ideology of Zionism and based on biblical precedent,

¹⁶ See Daniel J. Elazar and Shmuel Sandler, eds., *Israel's Odd Couple: The Nineteen Eighty-Four Knesset Elections and the National Unity Government* (Detroit, Mich., 1990).

¹⁷ See Bernard Reich and Gershon R. Kieval, eds., *Israeli Politics in the 1990s: Key Domestic and Foreign Policy Factors* (Westport, Conn., 1991).

¹⁸ Jacob M. Landau, *The Arabs in Israel: A Political Study* (London, 1969), presents a comprehensive survey and analysis of the role of the Arabs in Israel. An alternative perspective is provided by Sabri Jiryis, *The Arabs in Israel* (New York, 1976). See also Ian Lustick, *Arabs in the Jewish State: Israel's Control of a National Minority* (Austin, Tex., 1980).

has been a central feature of Israeli life and, before independence, in the ideology of the Zionist movement and the Yishuv.

Jewish Zionist immigration to Palestine in the nineteenth century came predominantly from Central and Eastern Europe, especially Russia and Poland. The early immigrants laid the foundations for an essentially European culture in Palestine, and subsequent immigrants accelerated the trend. In the immediate aftermath of World War II and the Holocaust, additional Jewish immigrants arrived in large numbers from the countries of Europe ravaged by war and from the concentration camps of Nazi Germany, adding to the high proportion of Ashkenazis among the Jewish population of Israel upon independence. These were soon followed by massive migrations from the Arab and Muslim states of the Middle East and North Africa (Sephardic or Oriental Jews—*Edot Hamizrach*, the Peoples of the East). This latter migration not only essentially removed the historic Jewish communities from the Arab states where they had resided for centuries but also altered (albeit not immediately) the demographic balance in the Jewish state. It created a situation in which a large portion of the population had societal and cultural traditions different from those of their Western counterparts, who constituted the majority of Israel's population and dominated its institutions. Eventually, Oriental Jews came to constitute a majority of the population and increasingly assumed positions of authority and responsibility within the Jewish state.

Although the ingathering of the exiles has involved immigration from numerous countries and was dominated by Oriental Jews, by 1990 the largest groups of immigrants were Jews from the Soviet Union (more than 185,000 arrived in 1990). Ethiopian Jews (Falashas) accounted for dramatic influxes in 1984–1985 (Operation Moses) and 1991 (Operation Solomon), when the last major segments of that ancient community were brought to Israel.

Israel is a Jewish state, and the Declaration of Independence pledges that "it will guarantee freedom of religion, conscience, language, education and culture; it will safeguard the Holy Places of all religions." Israel traces its links to the Land of Israel as the ancient birthplace of the Jewish people. The earliest connections between the Jews and the Land of Israel are recorded in the Bible, when Abraham, the first of the patriarchs of Judaism, migrated to the land God promised to him and his descendants.

Israel's Jews are of a single religious faith and share a spiritual heritage and elements of historical experience, but they are ethnically and culturally heterogeneous. Despite the guarantee by the Jewish state to its citizens of freedom of religion and conscience, there have been tensions and often open clashes between the religious and secular segments of the Jewish community, as they grapple with the contentious issues of an appropriate relationship between religion and the state and between the religious and secular authorities.¹⁹ These issues are symbolically embodied in the controversy over "who is a Jew," which has had philosophical, theological, political, and ideological overtones with specific practical dimensions. Because Jews have special positions under the terms of some

¹⁹ See Charles S. Liebman and Eliezer Don-Yehiya, *Civil Religion in Israel: Traditional Judaism and Political Culture in the Jewish State* (Berkeley, Calif., 1983).

laws (such as the Law of Return), it has become necessary to determine who is a Jew and to decide who makes such determinations. In the early years, a broad and liberal interpretation was fostered, essentially permitting each individual to decide. The Orthodox leadership, on the other hand, has sought to restrict Jewish identification and to ensure its conformity with Jewish law as determined by traditional Orthodox authorities. This division continues, with occasional celebrated instances punctuating the debate.

The concept of a Jewish state raises other questions for Israel's domestic and foreign policy, including the appropriate role for the non-Jewish population. Another is the related question concerning the status of the territories occupied by Israel in the 1967 war, especially the West Bank and the Gaza Strip. Should Israel retain the territories and their largely Palestinian Arab populations and thus raise questions concerning the "Jewishness" of the state, or should it take actions (such as "population transfers," i.e., expulsion or denial of full political participation) that might ensure the Jewishness of the state but raise questions about its democratic nature?

THE DOMINANT THEME OF ISRAEL'S HISTORY is the Arab-Israeli conflict, marked by six major wars and numerous efforts to achieve peace.²⁰ Israel's War of Independence (from the proclamation of independence to the armistice agreements of 1949) was followed not by peace and the normalization of its position in the region but by a refusal of the Arab states to accept its existence. The armistice agreements that ended the Arab-Israeli War of 1948–1949 did not resolve the central issues. The refusal of the Arab states even to consider negotiations with Israel (let alone acceptance of the existence of a Jewish state in Palestine) precluded a resolution of the Arab-Israeli conflict. And, while there were sporadic efforts to achieve a settlement, there was no sustained movement to achieve peace. In the early 1950s, tension continued to increase, clashes between opposing forces were frequent along Israel's frontiers, and the situation worsened because of external arms supply to the antagonists. The frontier between Egypt (and the Gaza Strip) and Israel became a flash point as Arab fedayeen (commandos) raided Israel from the Gaza Strip and Israel responded. Israeli ships were barred from the Suez Canal and its port of Eilat because of Egypt's closure of the Gulf of Aqaba at the Strait of Tiran.

In the summer of 1956, Egyptian President Nasser nationalized the Suez Canal, and tension in the region increased further. As part of a preconceived plan with the United Kingdom and France, Israel invaded the Sinai Peninsula in late

²⁰ The literature on the conflict is massive. These citations provide an introduction to the issue in documents and maps: Martin Gilbert, *The Arab-Israeli Conflict: Its History in Maps*, 3d edn. (London, 1979); and John Norton Moore, ed., *The Arab-Israeli Conflict: Readings and Documents*, abridged and rev. edn. (Princeton, N.J., 1977). My book, *Quest for Peace: United States-Israel Relations and the Arab-Israeli Conflict* (New Brunswick, N.J., 1977), deals with Israel's relations with the United States in the context of the efforts to resolve the Arab-Israeli conflict. It provides substantial bibliographical citations in the notes and bibliography to facilitate further study of the issue. See also Chaim Herzog, *The Arab-Israeli Wars: War and Peace in the Middle East* (London, 1982); and Trevor N. Dupuy, *Elusive Victory: The Arab-Israeli Wars, 1947–1974* (New York, 1978).

October to destroy hostile Egyptian military positions and, in a brief war, captured the Peninsula and Gaza Strip.²¹ The British and French governments issued an ultimatum and interposed their forces between Israel and Egypt along the Suez Canal. Eventually, under heavy pressure by the United States, Israel withdrew from Egyptian territory and the Gaza Strip. The U.N. Emergency Force (UNEF) was stationed on the frontier between the two states and helped ensure a quiet border for the next decade. The sea lanes through the Strait of Tiran were opened to Israeli shipping. But the hope that peace talks might follow was not realized, and, although the other Arab states did not join in the hostilities, neither did they try to reach a peace agreement with Israel. Their territories continued to serve as bases for attacks across the border into Israel.

In 1966 and 1967, border and terrorist incidents and retaliatory acts across the armistice lines increased and became more serious, especially between Israel and Syria. Then, in May 1967, Nasser called for the removal of UNEF from the armistice lines and from Sharm al-Sheikh in the Sinai Peninsula, mobilized the Egyptian military, and moved troops and equipment into the Peninsula. Nasser also announced the closing of the Strait of Tiran to Israeli shipping, thereby blockading the port of Eilat. Other factors, including the signing by Egypt and Jordan of a mutual defense treaty, contributed to the growing tensions. Finally, on June 5, 1967, Israel launched a preemptive military strike against Egypt. Other Arab states joined in the hostilities, which spread to include Jordan, Syria, and Iraq, among other Arab participants.²²

In the Six-Day War of June 1967, realities of Arab hostility, the nature of the Arab threat, and the difficulties of achieving a settlement became more obvious to Israelis. At the same time, the issues of the conflict changed with the extent of the Israeli victory: after six days of fighting, Israel had defeated the Arab armies and gained control of a substantial amount of additional territory, including the Sinai Peninsula, the Gaza Strip, the West Bank, East Jerusalem, and the Golan Heights. The Arab world now focused on regaining control of these areas. Progress toward peace was not made in the postwar period, although some efforts seemed promising.

On November 22, 1967, the U.N. Security Council adopted Resolution 242, which, among other things, called for the establishment of a just and lasting peace based on Israeli withdrawal from territories occupied in the 1967 war and on the end of all states of belligerency, respect for the sovereignty of all states in the area, and the right to live in peace within secure and recognized boundaries. Despite some efforts at peace by Ambassador Gunnar Jarring,²³ the special U.N. representative, and eventually by the United States and the Big Four at the United Nations, no significant breakthroughs were achieved.

²¹ See Robert Henriques, *A Hundred Hours to Suez: An Account of Israel's Campaign in the Sinai Peninsula* (1957; New York, 1967); Edgar O'Ballance, *The Sinai Campaign, 1956* (London, 1959); and Moshe Dayan, *Diary of the Sinai Campaign* (New York, 1966).

²² See David Kimche and Dan Bawly, *The Six-Day War: Prologue and Aftermath* (New York, 1971), orig. pub. as *The Sandstorm* (1968); Edgar O'Ballance, *The Third Arab-Israeli War* (London, 1972); and Randolph S. Churchill and Winston S. Churchill, *The Six Day War* (London, 1967).

²³ See Bernard Reich, "The Jarring Mission and the Search for Peace in the Middle East," *Wiener Library Bulletin*, 26 (1972): 13–20.

In April 1969, Nasser launched the “war of attrition” against Israel along the Suez Canal.²⁴ It was ended by a U.S.-sponsored cease-fire in August 1970, but subsequent efforts by the United States to negotiate an interim agreement that would secure a disengagement of forces and open the Suez Canal to shipping failed to make any progress. Inexorably, the parties moved toward a new round of hostilities.

On October 6, 1973 (Yom Kippur, the Day of Atonement), Egyptian and Syrian military forces attacked Israeli positions along the Suez Canal and in the Golan Heights.²⁵ Despite initial Egyptian and Syrian advances, Israel pushed Syria back beyond the 1967 cease-fire line and crossed the Suez Canal to take a portion of its west bank in Egypt. The United States and the Soviet Union proved instrumental in securing a cease-fire.

The Yom Kippur War was followed by renewed and intensified efforts to achieve peace. U.N. Security Council Resolution 338 of October 1973 reaffirmed the centrality of Resolution 242 as the framework for peace and called, for the first time, for negotiations between the parties to achieve this end. The cease-fire that followed the October war did not end the clashes or significantly ease tensions. Although a Geneva peace conference convened in December 1973 to deal with the Arab-Israeli conflict, it was primarily a public event. The progress in dealing with the issues occurred in private negotiations. U.S. Secretary of State Henry Kissinger sought to stabilize the cease-fire agreements and to achieve military disengagement agreements between Israel and Egypt and Israel and Syria. He was successful in achieving the former in January 1974 and the latter in May of that year. Israel subsequently moved its forces back from the Suez Canal, and Egypt resumed full control of the waterway. A U.N. Disengagement Observer Force (UNDOF) took up positions on the Golan Heights between Israel and Syria. In September 1975, another U.S.-brokered agreement between Israel and Egypt (known as Sinai II) provided for a further withdrawal of Israeli forces in the Sinai Peninsula, a limitation of forces in the area, and observation posts including those staffed by U.S. civilians. The United States undertook a number of commitments to both Egypt and Israel to help narrow the gap between the two parties and facilitate the agreement.

In November 1977, President Sadat broke the cycle of hostilities by announcing that he was prepared to negotiate peace directly with Israel. He subsequently visited Jerusalem at the invitation of Prime Minister Begin. During his visit, which included a speech before the Knesset, the Egyptian leader created a new psychological climate in the Middle East in which peace between Israel and its Arab neighbors seemed a realistic possibility. By this act, Sadat recognized Israel's right to exist and established the basis for direct negotiations between Egypt and Israel.

In September 1978, U.S. President Jimmy Carter invited Sadat and Begin to meet with him at the presidential retreat Camp David in Maryland, where they

²⁴ See Yaacov Bar-Siman-Tov, *The Israeli-Egyptian War of Attrition, 1969–1970: A Case-Study of Limited Local War* (New York, 1980).

²⁵ See Chaim Herzog, *The War of Atonement: October, 1973* (Boston, 1975); Edgar O'Ballance, *No Victor, No Vanquished: The Yom Kippur War* (San Rafael, Calif., 1978); and *The Sunday Times, Insight on the Middle East War* (London, 1974).

agreed on a framework for peace between Israel and Egypt and for a comprehensive peace in the Middle East.²⁶ The Camp David Accords set out broad principles for negotiations between Israel and the Arab states, including guidelines for a West Bank–Gaza Strip transitional regime with full autonomy and for a peace treaty between Egypt and Israel. The Egypt-Israel Peace Treaty was signed at the White House in Washington, D.C., on March 26, 1979, by Begin and Sadat, with Carter as witness. They agreed that negotiations on a transitional regime of autonomy for the West Bank and Gaza would begin one month after ratification.

The Egypt-Israel Peace Treaty represented a significant step toward resolution of the Arab-Israeli conflict, and the process of normalization of relations between Egypt and Israel moved ahead on schedule and without major disturbances. Normal relations between Egypt and Israel began officially on January 26, 1980, when Israel completed its withdrawal from two-thirds of the Sinai Peninsula, as called for in the peace treaty, and land, air, and sea borders between the two states were opened. In late February, embassies were opened in Cairo and Tel Aviv. In accordance with the terms of the treaty, Israel completed the return of the Sinai Peninsula to Egypt in April 1982. In 1989, the governments of Israel and Egypt concluded an agreement that resolved the status of Taba, a resort area on the Gulf of Aqaba, whose location had been a point of controversy. This concluded the land transfers required by the peace treaty.

Despite its smooth implementation, the treaty with Egypt was not followed by additional achievements on other frontiers, where tensions remained and problems often emerged. The 1982 War in Lebanon was the sixth major round of Arab-Israeli hostilities.²⁷ The frontier with Lebanon had been relatively quiet between Israel's War of Independence and the early 1970s, when the Palestine Liberation Organization (PLO) was forced out of Jordan and ultimately took up positions in Lebanon. Cross-border raids by Palestinians into Israel and Israeli retaliations heightened tensions. In March 1978, Israeli forces crossed into Lebanon in Operation Litani. After the passage of Security Council Resolution 425, calling for Israeli withdrawal, and the creation of the U.N. Interim Force in Lebanon (UNIFIL), Israel withdrew its troops. In July 1981, after additional fighting between Israel and the Palestinians in Lebanon, a cease-fire was achieved. The continued presence in Lebanon of missiles that had been moved there by Syria in the spring of 1981 remained an Israeli concern, as were the continuing PLO attacks against Israeli and Jewish targets worldwide.

On June 6, 1982, following an assassination attempt on the Israeli ambassador in London, Israel launched a major military action against the PLO in Lebanon (called Operation Peace for Galilee). The primary objectives: to ensure security for northern Israel, to destroy the PLO infrastructure that had established a state within a state in Lebanon, and to eliminate a center of international terrorism and a base of operations from which Israel could be threatened. But the political

²⁶ See Moshe Dayan, *Breakthrough: A Personal Account of the Egypt-Israel Peace Negotiations* (New York, 1981); and Ezer Weizman, *The Battle for Peace* (New York, 1981).

²⁷ See Itamar Rabinovich, *The War for Lebanon, 1970–1983* (Ithaca, N.Y., 1984); and Ze'ev Schiff and Ehud Ya'ari, *Israel's Lebanon War*, Ina Friedman, ed. and trans. (New York, 1984).

objectives that focused on the PLO as a potential political factor in a settlement were not so precise. The PLO was forced to withdraw its forces from Lebanon in August 1982. In May 1983, with U.S. assistance, Israel and Lebanon reached an accord that set the stage for withdrawal of Israeli forces from Lebanon. The instruments of ratification were never exchanged, however, and in March 1984, Lebanon, under pressure from Syria, abrogated the agreement. Israel withdrew most of its troops from Lebanon in June 1985, maintaining a small residual force and an Israeli-supported militia in a "security zone" in southern Lebanon, regarded by Israel as a necessary buffer against attacks on its northern territory.

No significant progress toward peace was achieved in the remainder of the 1980s. Tensions continued and, in December 1987, a Palestinian uprising (*intifada*) in the West Bank and Gaza Strip added a new dimension to the Arab-Israeli conflict. The *intifada* focused attention on the plight of the Palestinians in the territories occupied by Israel in the 1967 war and generated a wave of pro-Palestinian sympathies. Although some efforts were made to advance peace, no breakthrough was achieved until the Gulf War with Iraq.

Although not an Arab-Israeli war, the crisis and hostilities resulting from the Iraqi invasion of Kuwait in August 1990 posed a major political-strategic challenge for Israel and set the stage for renewed efforts to resolve the Arab-Israeli conflict. Problems included the appropriate role for Israel during the crisis period and the build-up to conflict, as well as the question of an Israeli role in the anti-Iraqi, U.S.-led coalition during hostilities. Scud missile attacks on Israel proved a formidable challenge when Israel ultimately decided, primarily in deference to American entreaties, not to respond with military force. Israel was kept outside the coalition and did not participate in the war. With the end of the hostilities came a U.S. effort to move toward resolution of the Arab-Israeli conflict. Once again, the question was posed whether a dramatic transformation had occurred in strategic and political thinking in the Middle East that could provide a unique opportunity to achieve peace or whether, in the final analysis, little had really changed. Israeli skepticism, informed by its reading of history, suggested a wariness not shared by those seeking to promote the peace process. Nevertheless, Israel agreed to participate in a U.S.-initiated peace conference in the fall of 1991 that marked some progress in the wake of the invasion of Kuwait.

THE FAILURE TO ACHIEVE PEACE and the continuation of Arab hostility has fostered Israel's focus on national security.²⁸ Israel's security policy has taken the form of an effective military capability, namely the Israel Defense Forces (IDF),²⁹ to respond to the military threat and a substantial program to acquire the necessary military equipment. A major proportion of Israel's GNP and budget has been allocated to defense and defense-related expenditures and the development of a

²⁸ See Bernard Reich and Gershon R. Kieval, eds., *Israeli National Security Policy: Political Actors and Perspectives* (Westport, Conn., 1988).

²⁹ For an overview of the IDF, its background and development, see Edward Luttwak and Dan Horowitz, *The Israeli Army* (New York, 1975); and Ze'ev Schiff, *A History of the Israeli Army (1870-1974)*, Rafael Rothstein, trans. (New York, 1974); for further research, see Jehuda L. Wallach, *Israeli Military History: A Guide to the Sources* (New York, 1984).

substantial defense production capability.³⁰ At the same time, Israel has seen the need to acquire supplies from a number of other states, in the early years primarily from France and in recent decades overwhelmingly from the United States.

Israel's isolation within the Middle East as a consequence of the continuing Arab-Israeli conflict has led it to focus attention in its foreign policy on the world beyond the Arab fence around it.³¹ Since independence, Israel has sought positive relations with members of the international community. It has joined and participated in the work of international organizations, and it has sought to establish and maintain friendly relations with as many states as possible.³² It has seen Europe and the developing world, especially Africa and Latin America, as important components of its overall policy. It has sought to maintain positive relations with Europe based on the commonality of the Judeo-Christian heritage and democratic tradition and the memories of the Holocaust. And, since its inception, Israel has sought to develop economic and political links with the European Community. Its approach to the developing world, which began in earnest in the late 1950s, focused primarily on Israel's ability to provide technical assistance to promote development.³³ Within the framework of this broad international effort, however, the focus has been on relations with the United States and the Soviet Union.

Israel has had a variable relationship with the Soviet Union and the members of the Eastern bloc since before its independence.³⁴ Although the Soviet Union supported the U.N. partition plan in 1947 and Israel's independence in 1948, relations deteriorated rapidly, and the Soviets shifted to a pro-Arab position by the mid-1950s, including economic assistance and arms supply complemented by diplomatic and political support. In 1967, the Soviet Union and the Eastern bloc states, with the exception of Romania, broke diplomatic relations with Israel. In

³⁰ See Stewart Reiser, *The Israeli Arms Industry: Foreign Policy, Arms Transfers, and Military Doctrine of a Small State* (New York, 1989).

³¹ An invaluable source on Israeli foreign policy is *Israel's Foreign Relations*, volumes of selected documents issued by the Ministry for Foreign Affairs. Volume 1 (issued in 1976) begins in 1947. Volume 8 (issued in 1990) covers the period from 1982 to 1984. The Israel State Archives and the Central Zionist Archives of the World Zionist Organization have issued volumes of *Political and Diplomatic Documents* beginning with December 1947 (issued in 1979) that are akin to the *Foreign Relations of the United States* series of declassified official U.S. materials. For overviews of Israel's foreign policy, see Aaron S. Klieman, *Israel & the World after 40 Years* (Oxford, 1989); Gideon Rafael, *Destination Peace: Three Decades of Israeli Foreign Policy* (New York, 1981); and Walter Eytan, *The First Ten Years: A Diplomatic History of Israel* (New York, 1958). Michael Brecher, in *Decisions in Israel's Foreign Policy* (New Haven, Conn., 1975); and *The Foreign Policy System of Israel: Setting, Images, Process* (New Haven, 1972), provides a comprehensive approach to Israel's foreign policy process and examines some major decisions.

³² The relationship with the United Nations has been ambivalent. On the one hand, that organization was instrumental in the creation of the state and was generally supportive during the first two decades. Since 1967, however, and especially since the 1973 war, Israel has regarded the United Nations as an essentially negative factor inimical to its interests. See *Israel and the United Nations*, report of a study group set up by the Hebrew University of Jerusalem (New York, 1956); and Avi Beker, *The United Nations and Israel: From Recognition to Reprehension* (Lexington, Mass., 1988).

³³ Israel's international cooperation program is discussed in Leopold Laufer, *Israel and the Developing Countries: New Approaches to Cooperation* (New York, 1967); and Shimeon Amir, *Israel's Development Cooperation with Africa, Asia, and Latin America* (New York, 1974).

³⁴ See Arthur Jay Klinghoffer with Judith Apter, *Israel and the Soviet Union: Alienation or Reconciliation?* (Boulder, Colo., 1985); and Arnold P. Krammer, *The Forgotten Friendship: Israel and the Soviet Bloc, 1947-53* (Urbana, Ill., 1974).

subsequent years, the questions of the Soviet role in the Arab-Israeli conflict and the peace process and the status of Jews in the Soviet Union became central themes in Israeli-Soviet relations. During the tenure of Mikhail Gorbachev, a thaw developed, and relations between Israel and the Soviet camp improved in a number of spheres. With the collapse of the Soviet bloc, the termination of the Warsaw Pact, the formal declaration of the end of the cold war, and the growing democratization of Eastern Europe, individual states restored diplomatic relations with Israel. The Soviet Union opened the way for a massive emigration of Soviet Jews (who overwhelmingly went to Israel) and improved its ties with the Jewish state, formally restoring diplomatic relations in October 1991.

The links between Israel and the United States have been central to Israeli foreign and security policy and a cornerstone of U.S. Middle East policy.³⁵ The special relationship between the United States and Israel developed slowly, as initial indifference gave way to sympathy and support based on intangible elements of biblical tradition, the Holocaust, and a perception of Israel as a like-minded pioneering state with a Western-style, liberal democracy. This special relationship remains complex and multifaceted while undergoing expansion and evolution over more than four decades. The links between the two states revolve around a broadly conceived ideological agreement and continue to be based on substantial positive perception and sentiment evident in public opinion and official statements and manifest in political-diplomatic support and military and economic assistance. But these relations have not been enshrined in a legally binding commitment joining the two states in a formal alliance. The two states maintain a remarkable degree of parallelism and congruence on such broad objectives as the need to prevent major war, to resolve the Arab-Israeli conflict, and Israel's economic and social well-being. But noncongruence of U.S. and Israeli policy on specific issues that have derived from differences of perspective continues. Nevertheless, Israelis see the United States as their ultimate protector and the primary source of their sophisticated military hardware. The United States is an indispensable ally that provides Israel with economic, technical, military, political, diplomatic, and moral support.

THE THEMES DISCUSSED HERE seem likely to continue to be dominant in the decade of the 1990s. Israel will continue to seek peace with its Arab neighbors but will be prepared for conflict in its absence. It will seek to expand its international relationships but will continue its focus on the superpowers, especially the United States, which remains critical for Israel in every area of its concerns. Domestic politics will be intense and divisive as Israel's traditional political parties and elite will be joined by newcomers vying for positions of political leadership and as Israel seeks to absorb and integrate new immigrants, to develop an economically viable state in the Middle East, and to define itself as a Jewish state.

³⁵ See Bernard Reich, *The United States and Israel: Influence in the Special Relationship* (New York, 1984); and Nadav Safran, *The United States and Israel* (Cambridge, Mass., 1963).

SHAUL BAKHASH

THE ISLAMIC REVOLUTION OF 1979 not only generated its own controversies among historians but also brought into sharper focus earlier debates on issues in nineteenth and twentieth-century Iranian history. The literature on the history of Iran over the last two centuries is not extensive. Archival and documentary sources are limited, particularly for the nineteenth and the first half of the twentieth centuries, and the scholars who have worked in the field are few. Thus the critical mass of scholars and studies necessary to give rise to meaningful controversies among historians, or to generate new research once problems have been identified, has generally been lacking. Even when scholars have differed on the interpretation of the data, they have not often used articles in scholarly journals to address these differences directly. Nevertheless, a number of interesting issues have emerged, some of which this article will examine: the political implications of the Shi'i doctrine of the state and the role of the Shi'i ulama, or religious leaders, in the great political movements of the last two centuries; those who suffered or benefited from economic development since the mid-nineteenth century and the overall impact of economic modernization on Iranian society; and the causes of the Islamic Revolution of 1979.

Many historians have noted the prominence of the ulama in major movements of political protest since the nineteenth century, including the tobacco protest movement of 1890–1891, the constitutional revolution of 1905–1911, the oil nationalization crisis of 1951–1953, the uprising ignited by the arrest of Ayatollah Ruhollah Khomeini in June 1963, and the Islamic Revolution of 1979. In a much-cited article, Ann K. S. Lambton, a historian who has written extensively on aspects of institutional and social history, the structure of the state, and political thought in Iran in the Islamic period, ascribed this oppositional role of the Shi'i ulama at least in part to the Twelver Shi'i doctrine of the state and the related doctrine of the Hidden Imam.¹ Twelver Shi'ism—the form of Shi'ism dominant in Iran—holds that the leadership of the community passed from the Prophet Muhammad to a series of infallible imams, the twelfth of whom, the Mahdi, removed himself from this world in the ninth century. The Hidden Imam (or the Lord of the Age, as he is also known) will return in the fullness of time to establish the government of truth and justice. Shi'i jurists also held that during the absence

I would like to thank my colleague, Jack R. Censer, who read and commented on an earlier version of this article.

¹ Ann K. S. Lambton, "Quis Custodiet Custodes: Some Reflections on the Persian Theory of Government," *Studia islamica*, 5 (1956): 125–48; and 6 (1956): 125–46.

of the Hidden Imam, the vice-regency of the Imam and therefore the mantle of leadership of the community fell to the jurists, or *mujtahids*, themselves. Until Khomeini, however, the jurists tended to treat the idea that the *mujtahids* were the true leaders of the community largely as a theoretical claim rather than an actual mandate to assume political power and to rule.

Lambton noted that the thrust of the Shi'i concept of the state, at least in theory, was to render all government except that established by the Hidden Imam illegitimate. Lambton did not argue that the doctrine of the Hidden Imam inclined the clerics toward unwavering opposition to the state, much less to revolution. On the contrary, as she stated in a number of articles, while tension often existed in the relations between the religious establishment and the government, the attitude of individual jurists and members of the clerical community to the state tended to vary widely.² Because the state was by definition associated with tyranny and unjust rule, many avoided any association with it; others did not withhold their cooperation, either because they sought power and personal enrichment or because they believed such cooperation or at least toleration of the ruler was necessary to permit the life of the community to continue and to allow the believer to live according to Muslim law and precepts. Moreover, Lambton argued, while the clerics were on occasion moved to speak out against tyranny and oppression, in joining and even leading opposition movements such as the tobacco protest movement and the constitutional revolution the clerics intended to end oppression, defend Islam, and protect Iran against foreign encroachment, not to bring about revolution. Their aim was to restore the traditional Islamic restraints on the ruler and was at most reformist, not revolutionary.

Other scholars have built on Lambton's work and reached broadly similar conclusions but with a somewhat different emphasis. The American historian Nikki Keddie, for example, like Lambton, has emphasized the conditions (misrule, foreign encroachment) that aroused clerical antagonism to the Qajars (1779–1925). But Keddie located the heightened political and oppositional role of the ulama in the nineteenth century not only in traditional Shi'i doctrine but more specifically in doctrinal developments in the late eighteenth and early nineteenth centuries, when the *usuli* school, which claimed for the *mujtahids* greater prerogatives in interpreting Islamic law and guiding the community, triumphed over the *akhhari* school, whose claims on behalf of the *mujtahids* were far more modest. Also, while Keddie followed Lambton in stressing the often limited political aims of the clerics, even in active opposition, she depicted the clerics in the nineteenth (and the twentieth) century as far more independent of the state, more politically aware and active, more persistently opposed to the government, and more revolutionary, than did Lambton.³

² See, for example, A. K. S. Lambton, "The Persian 'Ulama and Constitutional Reform," in T. Fahd, ed., *Le Shi'ism imamite* (Paris, 1970), 245–69; and "The Persian Constitutional Revolution of 1905–06," in P. J. Vatikiotis, ed., *Revolutions in the Middle East* (London, 1972), 173–82.

³ For Keddie, see especially Nikki R. Keddie, *Religion and Rebellion in Iran: The Tobacco Protest of 1891–1892* (London, 1966); as well as "The Origins of the Religious-Radical Alliance in Iran," and "Religion and Society in Iran," both in Nikki R. Keddie, *Iran: Religion, Politics and Society* (London, 1980), 53–65 and 81–116.

Hamid Algar, a historian whose first book emphasized what the author saw as the leading role of the ulama in defending the population against Qajar tyranny in the nineteenth century, pushed Keddie's argument even further.⁴ Lambton treated the Shi'i view that all states are essentially usurpatory as facilitating opposition to the government in times of political crisis. In Algar's hands, this concept became central in defining the essential, conscious attitude of the religious leaders and community of believers to the state. He also treated it as a formula for vigilant political activism. The doctrine, he argued, required Shi'is to strive actively to limit tyranny and end injustice. Moreover, while Keddie saw the mass protest movements of the nineteenth and twentieth centuries as resulting from an alliance among the clerics, secular radicals influenced by Western ideas, and Islamic reformers, Algar accorded to the clerics the central role in these opposition movements, attaching little importance to the role played by their secular allies. In his reading, it is the clerics who emerge in Iran's recent history as champions of the common people and popular causes. Unlike Lambton (or Keddie), Algar had little to say about the propensity of clerical leaders to cooperate with the state or about the self-interest or corruption that characterized many of them.

In addition, Algar injected the element of social justice into the interpretation of the doctrine of the Hidden Imam. A striving for social justice, he argued, is integral to the doctrine and thus can explain the involvement of religious leaders and other believers in opposition movements (an element that hardly figures in Lambton's explanation for clerical participation in nineteenth-century political agitation, for example). A vital aspect of the longing for a return of the Hidden Imam, Algar suggested, was an aspiration for social justice and a commitment to the cause of the downtrodden. The Hidden Imam was traditionally expected to inaugurate the reign of truth and justice, although this phrase has meant different things over the generations. Algar's reading entails a decidedly modern, twentieth-century definition of social justice. Finally, Algar interpreted Shi'i doctrine, particularly as it had evolved since the late eighteenth century, as authorizing the *mujtahids* to dispense guidance to the community not only on religious but on political matters as well.

This interpretation has been challenged by Said Arjomand, a sociologist interested in the implications of Shi'i political thought for clerical attitudes toward the state. In a book published in 1984,⁵ Arjomand set out to demolish the view that, in the eyes of the Shi'i scholars, the doctrine of the Hidden Imam implied the illegitimacy of all secular government or inclined the clerics to involve themselves in political affairs. Arjomand's book is concerned primarily with the era of the Safavids (1501–1722), when Shi'i Islam became the official state religion; but Arjomand clearly claimed far wider application for his findings. He argued that, for long stretches of history, Shi'i religious leaders accommodated themselves to

⁴ Hamid Algar's first book was *Religion and State in Iran 1785–1906: The Role of the Ulama in the Qajar Period* (Berkeley, Calif., 1972). The argument summarized here is made in "The Oppositional Role of the Ulama in Twentieth Century Iran," in Nikki Keddie, ed., *Scholars, Saints and Sufis: Muslim Religious Institutions in the Middle East since 1500* (Berkeley, 1972), 231–55.

⁵ Said Amir Arjomand, *The Shadow of God and the Hidden Imam: Religion, Political Order and Societal Change in Shi'ite Iran from the Beginning to 1890* (Chicago, 1984).

political authority, however tyrannical its character; that the doctrine of the Hidden Imam was developed in large part, if not primarily, to avoid friction with the rulers; and that, over the centuries, Shi'i clerics developed arguments discouraging rebellion even against oppressive rulers and advised political acquiescence, withdrawal from worldly affairs, or service to rulers in the interests of social order and the maintenance of the religious life of the community.

Arjomand asserted that the doctrine of the Hidden Imam was a formula for quiescence, not political action, and served as "a utopian substitute for political theory," "devoid of direct implications for political rule."⁶ By relegating the reign of the Lord of the Age to some indeterminate future, the doctrine served to legitimate, not to challenge, the state. The assertion that the true vice-regency belongs to the *mujtahids* remained a theoretical formulation rather than a serious claim to political authority.

Implicit in this dispute over the interpretation and political significance of religious doctrine was an argument, among a new generation of scholars like Algar and Arjomand, about contemporary politics, specifically the role of the Shi'i clerics in advancing "progressive" as opposed to "reactionary" causes and also the competing political claims of Iran's religious leaders and secular nationalists. Both these groups at various times have claimed to champion the cause of democracy, liberty, and social justice. The debate among scholars predates the Islamic Revolution but has continued and acquired fresh relevance as a result of it. Algar, as we have seen, cast the ulama, armed with the principles of the Shi'i faith, in this desirable role. His 1972 article, "The Oppositional Role of the Ulama in Twentieth Century Iran," proved to be prescient regarding the manner in which the Islamic movement in Iran, under the leadership of Khomeini and those inspired by him, would interpret the role of religion in politics, the meaning of the doctrine of the Hidden Imam, and their own mission in opposing the autocratic rule of Mohammad Reza Shah.⁷ But it is also clear that what Algar treated as the essence of the Shi'i attitude toward the state was but a 1970s radical reformulation of an older tradition—a reformulation that emphasized political activism aimed at achieving social justice and imposing constitutional limits on governance by secular rulers. Under the influence of this new reading, Algar in fact revised his own earlier view, that in supporting the constitutional revolution of 1906 and the liberal and largely secular constitution which it produced, "the ulama failed to perceive the nature of what was being demanded and its implications for Iran and themselves."⁸ Arjomand, on the other hand, depicted the religious leaders as lacking strong political motivation, treated their involvement in opposition politics as an aberration rather than inherent in Shi'i doctrine, and saw in their populism a tendency toward obscurantism, fanaticism, and reaction.

This (unacknowledged) aspect of the debate, with its overtones of relevance to the politics of contemporary Iran, was made explicit in 1983, in an essay by Willem Floor suggestively titled "The Revolutionary Character of the Ulama:

⁶ Arjomand, *Shadow of God*, 38.

⁷ See note 4, above.

⁸ Algar, *Religion and State in Iran*, 259.

Wishful Thinking or Reality?"⁹ Focusing on the period since 1951, Floor argued that the ulama had been part of the traditional Iranian power elite and had for the most part supported the existing social order. Moreover, they had consistently stood against change and social reform since the nineteenth century; their opposition to the state in the nineteenth century was largely aimed at a return to traditional values and the traditional order. Floor noted that, in 1953, the ulama opposed the nationalist prime minister, Mohammad Mossadegh, "who promised fundamental changes in Iranian society,"¹⁰ and threw their support behind the shah at a crucial moment in the latter stages of the oil nationalization crisis. They opposed government-sponsored land distribution legislation both in 1960 and 1962 and the extension of suffrage to women in 1962. They frequently failed to join other groups in opposing royal dictatorship or in demanding the kind of structural change that would end the exploitation of a majority of the population. According to Floor, the involvement of the ulama in politics in the 1960s and 1970s was motivated not by concern for the poor or by high principle but primarily by the threat posed to the material interests and power base of the religious leaders by a centralizing state.

The debate has thus shifted from theory to practice, from an argument regarding the political implications of the Shi'i doctrine of the state to one regarding the actual role of the religious leaders in the political arena. Algar provided a closely argued and finely nuanced exposition of the manner in which the doctrine of the Hidden Imam had come to be interpreted by certain religious thinkers in Iran during the 1970s. But it was a view overly respectful of the role of the ulama in the political sphere. Algar saw in the Shi'i attitude to the state an inherent inclination to oppose tyranny and promote social justice and constitutional guarantees. He failed to take into account what others who were skeptical of the inclinations of the clerics and the majority of their followers foresaw: that, whatever the theory, the clerics in power would prove equally if not more tyrannical, ruthless, and corrupt than the secular rulers they replaced. Arjomand's thesis acted as a useful corrective to Algar's interpretation, but it was problematic in two ways. First, Arjomand seemed to insist on one "correct" interpretation of the Shi'i attitude to the state. Whatever did not fit this interpretation he treated as an aberration. But this meant that Arjomand had difficulty explaining the recurring, politically activist pattern of ulama behavior. Arjomand failed sufficiently to emphasize that there are different strains to the Shi'i attitude to the state or to recognize that Shi'i doctrine can be mined to justify and encourage active opposition to the state as readily as to justify political quiescence in the face of oppression. Second, given his view that the doctrine of the Hidden Imam was "devoid of direct implications for political rule," Arjomand was left with the task of explaining how it was possible in 1979 for the ulama to lead a revolution, overthrow the monarchy, and use Shi'i doctrine to establish a

⁹ Willem Floor, "The Revolutionary Character of the Ulama: Wishful Thinking or Reality?" in Nikki R. Keddie, ed., *Religion and Politics in Iran: Shi'ism from Quietism to Revolution* (New Haven, Conn., 1983), 73–97.

¹⁰ Floor, "Revolutionary Character," 93.

government in which supreme power is vested exclusively in the religious leadership.

Arjomand's answer, in a second book, has been to argue that what occurred as a result of Khomeini's teaching and the movement that brought Khomeini to power was a "revolution" in Shi'i political thought and in the attitude of the ulama toward the state.¹¹ A radical reformulation of the traditional understanding of Shi'i political doctrine certainly has occurred. There is virtually no precedent for the Shi'i ulama to assume power and take charge of the government themselves; and the generally accepted interpretation of traditional doctrine did not suggest such an assumption of power, although there existed other, competing interpretations. Even Algar's article in 1972, which reflected the reformulation of the traditional doctrine already taking place in the 1970s, did not argue that the *mujtahids* were claiming a right to assume power, rather, that the traditional doctrine required believers and religious leaders to strive to limit the tyranny and misrule of a secular ruler through constitutional guarantees and similar checks on governmental authority.

But the kernel of the idea that rightful rule belonged to the *mujtahids*, and the potential for such an interpretation of the traditional theory of the state, did exist. Arjomand himself had cited (but not dwelt on) examples of Safavid jurists who in the seventeenth century claimed the vice-regency of the Hidden Imam (and, by implication, the right to rule) for a qualified *mujtahid*.¹² The *usuli* victory over the *akhbari* school in the last century, as we have seen, suggested far greater political activism by the clerics, and the clerics seized the political initiative in periods of crisis on a number of occasions in the nineteenth and twentieth centuries. In the 1970s, Khomeini successfully argued for the practical application of what, until then, had been merely theory. This was a possibility not foreseen in Arjomand's original account of the Shi'i doctrine of the state. Thus, in his second book, Arjomand modified his earlier view. He conceded that certain features of Shi'i Islam and its organizing myths, such as the centrality of martyrdom, were powerful instruments for the political mobilization of the common people and oppositional politics and that although the jurists in earlier times may have succeeded in containing the millenarian tradition in Shi'ism, they had failed to eradicate it. Thus for Arjomand, too, a highly politicized Shi'ism characterizes present-day Iran, although he sees this as a "revolution" rather than an evolution, however rapid, of existing doctrine.¹³

¹¹ Said Amir Arjomand, *The Turban for the Crown: The Islamic Revolution in Iran* (New York, 1988), 177–88.

¹² Arjomand, *Shadow of God*, 185–86.

¹³ A. A. Sachedina tries to show, in *The Just Ruler in Shi'ite Islam* (New York, 1988), that leading Shi'i jurists from early Islamic times to the present have consistently argued that the vice-regency of the Hidden Imam belongs to a qualified *mujtahid*, who should assume the reins of power. But the author's own quotations from the sources often belie his central thesis. The argument of his book and his interpretation of the sources has also been strongly questioned in reviews. See especially the reviews of *The Just Ruler* by Said Arjomand, forthcoming in *Middle Eastern Studies*; and by Hossein Mudarressi, forthcoming in the Fall 1991 issue of the *Journal of the American Oriental Society*.

THE CONSTITUTIONAL REVOLUTION OF 1906 resulted from widespread opposition to the Qajars (1779–1925) and a protest movement in which the merchant classes and the clergy played a major role. It gave Iran a constitution, based on the Belgian constitution, which established limits on royal authority; recognized the will of the people, along with divine mandate, as a second source of sovereignty; created a popularly elected Majlis, or parliament; and vested in the legislature the power to make all laws. The constitutional revolution has traditionally been explained in terms of the ineptitude and corruption of the Qajars, growing unease and resentment among politically aware Iranians over foreign domination and interference, fear that the concession-granting policy of the Qajars would undermine Islam and the national identity of the Iranians, and the detrimental impact of Qajar economic policies on the majority of the population and especially the merchant classes and the bazaar traders and shopkeepers.

The role of the merchant classes in advancing the constitutional cause has particularly intrigued scholars. Keddie, for example, expounding a widely shared view, explained their support for the movement by alluding to tariff policies that favored foreign merchants, imports of finished goods that competed with domestic manufactures and undermined local crafts and industries, and a concession-granting policy, exemplified by the tobacco regime of 1891, which gave foreigners a monopoly over areas of trade traditionally controlled by Iranian merchants and traders.¹⁴ No one would dispute the broad outlines of this picture. But it implies an economy in decline and a merchant class debilitated by the overall impoverishment of the country and ground down by adverse government policy and the regime's inability to resist foreign encroachment.

In the late 1970s, an Israeli economic historian, Gad Gilbar, offered a radically different interpretation.¹⁵ First, Gilbar painted a picture of an economy in which trade was growing, the road and communications network improving, large-scale agriculture in cash crops profitable, the area under cultivation expanding, domestic industry finding its feet, and exports rising. If many crafts were in decline, some, like carpet weaving, were rapidly expanding. The European presence in Iran, he asserted, was limited and indirect (confined mostly to imports), thus the benefits of the expansion of foreign trade and domestic markets accrued mostly to native merchants and traders. Moreover, Gilbar cited figures that purported to show that, in certain parts of the country at least, prosperity was trickling down. Peasant-cultivators and sharecroppers were diversifying their diets and acquiring the means to purchase carpets and household comforts and to pay for their daughters' dowries.

Second, while Gilbar's analysis confirmed the view that the merchants sup-

¹⁴ Nikki R. Keddie, *Roots of Revolution: An Interpretative History of Modern Iran* (New Haven, Conn., 1981), 54–59. For another account of the destructive effects of foreign competition on domestic industries, especially textiles, see Ann K. S. Lambton, "Persian Trade under the Early Qajars," reprinted in Lambton, *Qajar Persia: Eleven Studies* (London, 1987), 108–39.

¹⁵ For Gad Gilbar, see especially "The Changing Economic Involvement of Iran with the West, 1800–1914," unpublished paper presented at the Conference on State, Society and Economy in Nineteenth Century Iran and the Ottoman Empire, held at Reza Shah Kabir University in Iran in 1978; "The Big Merchants (tujjar) and the Persian Constitutional Revolution of 1906," *Asian and African Studies*, 11 (1977): 275–303; and "Persian Agriculture in the Late Qajar Period, 1860–1906," *Asian and African Studies*, 12 (1978): 312–65.

ported the constitutional movement because government policies (customs administration, tax policy, foreign concessions) adversely affected their interests, he saw their situation in a different light. In his interpretation, the big merchants joined the constitutional movement not out of desperation but out of strength. He depicted a class of large-scale merchants that had grown wealthy, was expanding its trade and agricultural activities, and was increasingly self-confident. This class was resentful of the privileges enjoyed by foreign traders precisely because it now had the financial means and networks to undertake the activities previously in the hands of foreigners. These points have significant bearing on the interpretation of the causes of the constitutional revolution and suggest that the revolution occurred against the background of an expanding, not a declining economy. The revolution was made not only, for example, by displaced artisans and merchants losing out to foreign competition but at least in part by powerful merchants, testing their power and demanding a greater say in setting national policy. Gilbar's views have received support from the work of a number of other scholars.¹⁶

Keddie has continued to challenge this view. Increased consumption of tea, sugar, and tobacco, and increased cultivation of opium (especially if achieved at the expense of grain cultivation), she has argued, did not necessarily mean improved diets for the peasantry. Changes in crop patterns in favor of cash crops subjected Iranian villagers to the fluctuations of domestic and international market prices and made them more vulnerable to famine and scarcity. And if some merchants benefited from trade relations with the West, most did not.¹⁷

In a recent article, Keddie appears to have modified her views somewhat, agreeing that at least some members of the merchant class were not badly damaged by (even thrived on) the incorporation of Iran into foreign markets. She notes that, despite foreign competition, the native Iranian bazaar structure remained largely intact (although she attributes this to the limited volume of Iranian trade with Europe and the fewer European residents in Iran, as compared to Egypt and Turkey); that wealthy Iranian import-export and domestic merchants and other "economically dominant groups and classes," no longer believing that the Qajars adequately represented their interests, played an important role in the constitutional revolution; and that whether there was general impoverishment or an increase in prosperity in nineteenth-century Iran is a question on which there is legitimate dispute.¹⁸ However, it is also clear that Keddie has not altered her basic view that the overall economy and the majority of the population suffered as a result of changes brought about by Iran's

¹⁶ Guity Nashat also described a merchant class that was vigorous, expanding its activities, and highly prosperous in an unpublished paper on the merchants in nineteenth-century Iran presented to the conference on State, Society and Economy in Nineteenth Century Iran and the Ottoman Empire. Gilbar's account of expanding and profitable agricultural activity is borne out, at least for the region of Khuzistan, in the last quarter of the nineteenth century, in Mostafa Ansari's unpublished but instructive Ph.D. thesis, "A History of Khuzistan, 1878-1925" (University of Chicago, 1974).

¹⁷ Keddie, *Roots of Revolution*, 54-57. See also Nikki R. Keddie, "Iranian Revolutions in Comparative Perspective," *AHR*, 88 (June 1983): 583, n. 2.

¹⁸ Keddie, "Iranian Revolutions in Comparative Perspective," 583-85, 591-92. The phrase "economically dominant groups and classes" is on 591.

involvement in international trade. On balance, as she argued in 1981, entry into the world market was "more harmful than helpful to Iranians."¹⁹

Keddie's argument with Gilbar stems in part from her generally negative evaluation of the effects of imperialism on the economies of countries drawn into the international economic system in the nineteenth century and her general critique of the strategies of economic development that, under Western influence, have dominated in the Middle East and elsewhere in the Third World since World War II. Her skepticism about the argument that much worthwhile happened to the Iranian economy in the nineteenth century as a result of traffic with Europe is reflected also in her assessment of the results of strategies of economic development pursued under the two Pahlavi monarchs, Reza Shah (1925–1941) and Mohammad Reza Shah (1941–1979). Under Mohammad Reza Shah, beginning in 1954, the state sought to apply oil revenues and use large-scale government and private-sector investment to achieve a rapid expansion of the economy. The strategies adopted included heavy investment in infrastructure; import substitution; concentration on the domestic manufacture of mass and durable consumer goods; automobile and tractor assembly, and development of industries, including petrochemicals, steel, aluminum, and copper, that capitalized on Iran's resources of cheap oil, gas, iron ore, and other minerals; rapid expansion of educational opportunities; and land distribution combined with attempts at promotion of large-scale agriculture.

Keddie has little positive to say about the results of these development strategies. While she concedes that some worthwhile projects were implemented and some improvements took place, she argues that industrial investment was directed at showy projects, the private sector unreasonably coddled, and the emphasis on the provision of consumer goods excessive. She believes that such industries as were developed were inefficient, limited to assembly of imported parts, and excessively dependent on foreign suppliers. Although there was some trickle-down effect, she concludes that the real result of twenty years of planning and development was to increase gaps in income, to uproot villagers and cause massive migration to cities, to undermine domestic crafts, and to dislocate the economy. In her account, the mismanagement of the economy, not only in the years immediately before the revolution but over the long haul, remains an important explanation for the revolution.²⁰ Her assessment of the results of the land distribution program carried out under the old regime is equally negative.²¹

¹⁹ Keddie, *Roots of Revolution*, 56.

²⁰ Keddie, *Roots of Revolution*, 168–79.

²¹ Scholars have differed considerably in assessing the results of the land reform program carried out under the previous regime in the years 1963–1972. The debate deserves fuller treatment than is possible in this article. The critics, who include Keddie, Eric Hoogland, and Fred Halliday, fault the program for giving many beneficiaries of land distribution too little land to permit economic self-sufficiency, making no provision for the large population of pre-land reform agricultural workers (*khoshneshin*) who lacked sharecropping rights, and leaving too much land in the hands of large or middle-sized owners. A far more positive assessment of the results of land reform can be found in the work of Lambton, Charles Issawi, and Mohammad G. Majd. These authors stress as achievements the breakup of the large estates, the transformation of the land tenure system in Iran, the freeing of the peasantry from the yoke of large absentee owners, and the creation of a sizable class of middle-size peasants. I am personally inclined to agree with the latter group. Critics fault the reform for being insufficiently egalitarian; but, given the limited amount of arable land available, a

Keddie's assessment of Iran's economic performance in the 1960s and early 1970s has been challenged two fronts. Some scholars, notably Charles Issawi, while noting many of the problems identified by Keddie, reached markedly different conclusions regarding Iran's economic development in these years. Issawi, taking stock of economic development policies for the two decades after 1954, suggested that the trickle-down effect of Iran's economic expansion was considerable and that, by the mid-1970s, Iranians were enjoying considerably better standards of living than twenty years earlier. Moreover, he found the expansion of the economic infrastructure in Iran, the roads, railways, airport and port capacity, and telecommunications systems, impressive. Where Keddie was dismissive of Iranian industrialization, he saw evidence of important advances in import substitution and utilization of Iran's natural resources to develop an industrial base in oil, gas, petrochemicals, steel, and other metals. Issawi described the rate of industrial growth after 1960 as "almost unmatched in history." And where Keddie was almost entirely negative on Iranian economic planning under the government's various five-year plans, Issawi saw evidence of intelligent management of the economy in the 1960s and early 1970s. Issawi noted certain problems: income gaps seemed to be growing, even if most were better off, agricultural growth was not keeping pace with the growth in industry, urban areas were doing much better than the countryside. Nevertheless, the largely positive thrust of his analysis contrasts sharply with Keddie's largely negative one.²²

A different challenge to Keddie's assessment of Iran's economic performance in the two decades or so preceding the revolution (and, by extension, a different

thoroughly egalitarian reform would have given most peasants too little land or created uneconomic holdings; and to have succeeded in creating a class of middle-sized landowners (50 percent of the land, as a result of the reform, was in medium farms of 10 to 100 hectares) is no mean achievement.

For these various assessments, see Keddie, *Roots of Revolution*, 160–67; Fred Halliday, *Iran: Dictatorship and Development* (New York, 1979), 110–25; Eric Hooglund, *Land and Revolution in Iran 1960–1980* (Austin, Tex., 1982), esp. 77–122; Charles Issawi, "The Iranian Economy 1925–1975: Fifty Years of Economic Development," in George Lenczowski, ed., *Iran under the Pahlavis* (Stanford, Calif., 1978), 146–47, 163; Ann K. S. Lambton, *The Persian Land Reform 1962–1966* (Oxford, 1969); Mohammad G. Majd, "Land Reform Policies in Iran," *American Journal of Agricultural Economics*, 69 (1987): 844–48; and Majd, "Land Reform Policies in Iran: Reply," *American Journal of Agricultural Economics*, 71 (1989): 1050–53.

²² See Issawi, "Iranian Economy 1925–1975," 129–66. The quotation "almost unmatched in history" is on 150. Issawi was writing in 1975, just before the shah's disruptive attempt to stimulate massive economic growth by rapidly investing the huge revenues resulting from the oil price boom of 1973–1974. But Issawi noted the first signs of trouble, as indicated by rising inflation, a severe pressure on resources, and other developments. He warned of "strain . . . on the social fabric" and "potential dangers" (137). Issawi, however, distinguished carefully between Iran's economic performance and the management of the economy in the period up to 1973 and in the post-1973 oil-boom years. Jahangir Amuzegar, in the recently published *Dynamics of the Iranian Revolution: The Pahlavis' Triumph and Tragedy* (New York, 1991), makes a similar distinction. It seems to me that, in *Roots of Revolution*, Keddie blurs this important difference between the pre-1973 and post-1973 era. She tends to treat the pre-1973 decade, when inflation was low, growth rates impressive, improved standards of living everywhere evident, and the planners generally in control of their development plans, and the years that followed as part of an undifferentiated whole. Moreover, she does not quite resolve the implicit contradiction between depicting an economy in which economic benefits were confined to a narrow group and her assertion, in her 1983 article, "Iranian Revolutions in Comparative Perspective" (592) that policy under the Pahlavis had helped create a sizable, well-educated class of bureaucrats, technocrats, members of the professions, and industrialists who had sprung from humble origins.

conclusion regarding the economic sources of the revolution) can be found in Ervand Abrahamian's *Iran between Two Revolutions*, and his concept of "uneven development," which he applied to both the economic and the political spheres.²³ Like Issawi, and in even greater detail, Abrahamian provided extensive evidence of impressive economic progress in the twenty-five years before the revolution: the rapid growth of school and university enrollments, the industrial labor force, and the middle and professional classes; the expansion of literacy, health services, food consumption, and the production and sale of light consumer goods and consumer durables; the expansion of industry and commerce, the communications and infrastructure network; the large number of villagers who gained title to their own land under the land reform program. But he also provided figures for the secondary school graduates for whom there was no room in the universities, urban migrants who remained poorly housed, villagers who received inadequate or no land under the land distribution program, workers and employees whose pay remained low, and glaring gaps in income between the richest and the poorest groups in society. Economic development was "uneven" because the benefits of economic development were not evenly spread. While the rising aspirations of many were met, those of many others were not. In addition, Abrahamian argued, the development of political institutions did not keep pace with economic development. A growing population of urban migrants and educated Iranians, of members of the middle class and the industrial working class, had no means of expressing their political aspirations. Rapid economic development, albeit uneven, was accompanied by "political underdevelopment."

IN ADDITION TO ECONOMIC CAUSES OF THE ISLAMIC REVOLUTION, historians of Iran have debated a number of other possible explanations. These have ranged widely, and there is little agreement, except in a general way, about why the revolution occurred. A number of scholars have attempted to follow Ervand Abrahamian's class-based analysis of the Iranian Revolution, but historians (as well as sociologists and political scientists, who have also tackled the subject) cannot even agree on the classes that made or primarily benefited from the revolution. Those who have set out directly to address and evaluate the explanations of others, to arrive at one of their own, or to use the Iranian Revolution to address the issue of the theory of revolutions in general (à la Charles Tilly, Theda Skocpol, James C. Davies), have generally fallen back on an eclectic approach, offering a mixture of economic, social, cultural, and psychological explanations. A number of these scholars seem to feel that the Iranian case does not quite fit existing theories of revolution, which need to be revised in light of that experience.²⁴ It is impossible here to cover all

²³ Ervand Abrahamian, *Iran between Two Revolutions* (Princeton, N.J., 1982), 426–49.

²⁴ Three factors seem to account for this sense of Iranian exceptionalism. First, the Islamic Revolution was led and dominated by clerics. Second, in contrast to the other "great" revolutions (the English, French, Russian, and Chinese) the Iranian Revolution turned out to be religiously fundamentalist and "reactionary" rather than secular and "progressive" in orientation. Third, given the seemingly almost universal participation of Iranians of all classes and groups in the revolutionary movement, including those groups that had greatly benefited under the shah and would be hurt by the overthrow of the old regime, scholars have had a difficult time explaining the revolution in terms

this literature and not necessary to cover again material discussed by others. But I will touch on a number of issues in the debate not adequately examined elsewhere.

The eclectic or at least all-inclusive nature of many of the "explanations" of the revolution is a marked characteristic of much of this literature. In her *American Historical Review* article, for example, Nikki Keddie favored the theory of revolution advanced by Crane Brinton in *Anatomy of Revolution* and James C. Davies's J-curve theory of revolution. In Brinton, she was attracted to the argument that revolutions (as in England and France) are preceded by financial and economic crises that require accommodation with new groups, and also by the alienation of the intellectuals and elite from the court. In Davies, she found helpful the suggestion that revolutions are likely to occur when a period of sharp economic contraction and decline follows a period of considerable economic growth. But Keddie also found the "Marxist formula" to be "the closest socio-economic model for Iran's experience."²⁵

These explanations are not necessarily incompatible, but to argue that the Islamic Revolution occurred because the state no longer adequately represented the interests of the economically dominant groups and those who controlled and owned the basic means of production (which comes close to accepting Gilbar's explanation for the constitutional revolution) is somewhat different from arguing that the revolution occurred because of the grievances of the underprivileged classes. At the same time, Keddie seemed to place great emphasis on specifically Iranian cultural, religious, and national characteristics.²⁶ In addition, she cited such explanatory factors as economic grievances (income disparities, a discontented urban sub-proletariat, massive unemployment), political repression (arbitrary arrests, torture), the expansion of the middle class of bureaucrats, technocrats, and professionals, the discontent of a newly wealthy business and industrial class, and resentment at the foreign presence and influence in Iran. Such an all-encompassing explanation may not be misplaced in the case of the Iranian Revolution, but it does not make for a neat identification of causes, much less a "theory" of revolution.

Abrahamian applied primarily a class-based analysis, utilizing E. P. Thompson's neo-Marxist approach, to discuss the social basis of Iranian politics over the last

of class. For the argument that the Iranian Revolution can shed light on, or require revision of, existing theories of revolution, see especially Keddie, "Iranian Revolutions in Comparative Perspective," 579–80 and 591, n. 13; and Arjomand, *Turban for the Crown*, 189–210. Theda Skocpol has attempted to reconsider her theory of revolution, as expounded in *States and Social Revolution* (Cambridge, 1979), in light of the Iranian upheaval. See Theda Skocpol, "Rentier State and Shi'a Islam in the Iranian Revolution," *Theory and Society*, 11 (1982): 265–83. However, according to Arjomand (*Turban for the Crown*, 191), Skocpol does not adequately face the serious problem of reconciling the Iranian Revolution with the schema she proposed in her book.

²⁵ Keddie, "Iranian Revolutions in Comparative Perspective," 591.

²⁶ For example, she writes: "Iranians . . . in the last ninety years have engaged in an unusual number of large-scale popular revolts and revolutions . . . Indeed, Iran stands at the forefront of rebellious and revolutionary countries in the twentieth century—unmatched, to my knowledge, in the Muslim, Hindu, or Western world for the number and depth of its movements; only China, Vietnam, and possibly Russia provide competition"; and "Iranian Shi'i Islam may be the most resistant of all non-European religions to European culture"; Keddie, "Iranian Revolutions in Comparative Perspective," 580, 585.

century in *Iran between Two Revolutions*. His analysis of the coming of the Iranian Revolution, which is the focus of the concluding chapter, is also class-based and recalls, in some of its features, Georges Lefebvre's analysis of the coming of the French Revolution.²⁷ Keddie flirted with a class analysis but ended up depicting a situation in which all classes were discontented with the shah's rule. Abrahamian described a revolution primarily made by the "modern middle classes," composed of the intelligentsia, professionals, white-collar workers, and university students, as well as the industrial proletariat armed, as he put it, with a secular ideology. His overall analysis suggested that, in the end, it would be these classes who would capture the revolution. The fact that it was captured instead by the traditional clergy, armed with a religious ideology, and that a revolution which, he believed, was predominantly social, economic, and political should take on an ideological, predominantly religious form, Abrahamian attributed to a number of factors, some transitional, others more deeply rooted. But he assigned a critical role to Khomeini himself who, he wrote, was to the Iranian Revolution what Lenin, Mao, and Castro were to the Russian, Chinese, and Cuban revolutions. Khomeini's personality, ability to be all things to all social groups, and astute management of the protest movement allowed him to capture the leadership of the revolution. But Abrahamian suggested in the conclusion to his book that the discomfort of the modern classes would prove temporary, that they would in time "catch their breath," capitalize on popular discontent with the clerics, and reestablish their rightful leadership over the political process.²⁸ Whether the clerical regime has proven more durable than Abrahamian anticipated we do not know. In a more recent book, Abrahamian appears to have concluded that, whoever made the revolution, it is the traditional middle-class of bazaar merchants, petty traders, craftsmen, and clerics who have captured it and whose interests it represents.²⁹

Marvin Zonis, a University of Chicago political scientist who developed an interest in Jungian psychiatry (his most recent publication is a psychobiography of Mohammad Reza Shah), tried to deal in 1984 with what he saw as the inadequacies of a number of explanations of the Iranian Revolution. The explanatory scheme he proposed covers the usual economic and political causes encountered in other writers but leans most heavily on the factors of personality and psychology.³⁰ Zonis argued that the Iranian Revolution (and other revolutions) cannot be understood without adequate appreciation of the character of opposition leaders, the nature of opposition organizations, the potential for revolutionary mobilization of the urban population, and regime response. Zonis discussed

²⁷ In Lefebvre's view, the French Revolution can be seen as the outcome of an ever-widening movement of protest by different classes against the existing order. The revolution began with a limited and nonrevolutionary attempt by the French nobility to check the extension of royal power, but it widened into an attack on privilege as, in successive waves, the bourgeoisie, the urban working class, and the peasantry joined the protest movement. See *The Coming of the French Revolution*, Robert R. Palmer, trans. (Princeton, N.J., 1947). Abrahamian describes a process in which protests by the middle class and the intelligentsia are joined by the urban working class and then widen into a general rebellion against the old order.

²⁸ See Abrahamian, *Iran between Two Revolutions*, 530–37.

²⁹ See Ervand Abrahamian, *The Iranian Mojahedin* (New Haven, Conn., 1989), esp. 72–77.

³⁰ Marvin Zonis, "Iran: A Theory of Revolution from Accounts of the Revolution," *World Politics*, 35 (1983): 586–606.

only three of these factors in detail (and cited all four as issues that remain inadequately researched). But, even in his treatment of revolutionary mobilization and regime response, he favored personality and psychology as explanatory factors for the revolution.

For example, in assessing the potential of the urban population for revolutionary mobilization, Zonis added to the economic and political factors noted by others (dissatisfaction of the merchant and industrial classes with the shah's economic policies, the frustration deriving from the disappointment of rising expectations among the wider population, the suppression of all outlets for political opinion), factors that are purely psychological. In discussing the disappointment of rising expectations, he stressed feelings of "betrayal and frustration" among the people. Moreover, he wrote, the shah behaved in a manner so supercilious and lacking in compassion toward his own people that he added "feelings of injury and humiliation to those of frustration and betrayal . . . the behavior of the Shah increasingly came to be experienced as an insult—a narcissistic injury to his own people."³¹ Again, in assessing "regime response," Zonis focused almost exclusively on the personality of the shah; and, in analyzing it, he leaned heavily toward psychological factors. There was nothing inevitable about the outcome of the Iranian Revolution, he maintained, suggesting that the revolution could have been prevented, or its outcome altered, by more decisive regime response. Responsibility for the outcome of the revolution, he argued, ultimately rested with the shah, whom he depicted as a man lacking in the qualities of leadership. The shah lacked the capacity to deal with challenges or the willingness to fight and use force to save his throne. He suffered from a deep psychological dependence on the United States. Finally, his illness—the shah was suffering from cancer—not only may have reduced his cognitive and intellectual capacities but also left him feeling "personally threatened and depressed."³²

In seeking to explain why the revolution succeeded, a number of authors have focused on the "paralysis" of the state in the face of the widespread opposition that began to manifest itself in 1977 and grew to massive proportions in 1978–1979. For Gholam-Reza Afkhami, an Iranian political scientist and academic, who also had experience working in the Iranian government under the old regime, explaining this paralysis is the key to understanding the triumph of the revolution. That the economic dislocations of the post-1975 period should have led to discontent, protest, and demonstrations is hardly surprising. What requires explanation is the woeful inadequacy of the regime's response once the anti-government protests began. Afkhami believed that regime paralysis was to be explained by the very factors—centralization and concentration of power in the hands of the shah and his bureaucracy—that made for political stability and impressive gains in economic development and social transformation during the 1960s and early 1970s. By the mid-1970s, he wrote, urbanization, the spread of education, and the expansion of the middle class had created a society so differentiated and complex that continued progress required decentralization and wider political participation. For a number of reasons, however—primarily

³¹ Zonis, "Iran," 599–600.

³² Zonis, "Iran," 606.

the resistance of a large, pervasive, and self-interested bureaucracy and the role of supreme authority in which the shah had been cast by the very process of organizing for rapid economic growth and modernization—such decentralization and moves toward political participation could not occur. The state machinery lacked the capacity to respond to the revolutionary crisis, whether by force, concessions, or diversionary tactics, in a manner that would have made a difference; and the shah was precluded, by virtue of his role as the monarch of all Iranians and the symbol of national unity, from using brute force to crush the revolutionary movement.³³

In other words, Afkhami viewed the problem as systemic. Once the large-scale protests began in 1978, the revolution succeeded because it could not have been stopped, given the nature and organization of the Iranian state at that juncture. Afkhami is persuasive on the self-serving and inflexible nature of the bureaucracy. But his explanation of the revolution verges on the deterministic, suggesting a certain inevitability to both the structure that the state had assumed by the late 1970s and to the overthrow of the old regime. Strikingly absent from his analysis is any role for (and the assignment of any responsibility to) the principal actors on the regime side, particularly the shah. He depicted the shah as more acted upon than acting. His treatment of other principal decision-makers is similar, as if individuals were totally helpless to influence the course of events.³⁴

Arjomand was evidently also attracted by the argument that revolutions often owe more to the paralysis and collapse of the state than to the power of revolutionary groups. But he found existing theories regarding the causes for the collapse of states in revolutionary situations, particularly that advanced by Theda Skocpol, greatly inadequate. In Iran, he noted, the state did not lose its capacity for repression. The army and the security forces remained virtually intact and loyal to the shah until his departure from Iran in January 1979. Moreover, there was no defeat in war, no financial crisis. No peasant rebellions had taken place to cripple the regime. Thus none of the conditions thought necessary for the collapse of states was present. Arjomand remarked that this “leaves the usual generalizations about revolutions . . . mostly in the pits.” He concluded that in addition to the power of coercion, a system of authority also requires legitimacy to survive; and the shah lost his legitimacy when the members of the state bureaucracy and government organizations went on a massive, nationwide strike, paralyzed the government, and refused to recognize the shah’s authority.³⁵

³³ Gholam R. Afkhami, *The Iranian Revolution: Thanatos on a National Scale* (Washington, D.C., 1985).

³⁴ I made substantially the same point when I reviewed the book soon after it was published. (See *Middle East Journal*, 40 [1986]: 334–35). I argued that Afkhami’s view (that individuals—primarily, of course, the shah and his principal officers—were simply helpless to influence the course of events) was at odds with the blame he casts on the “liberal” politicians of the National Front and their followers for allowing themselves to be deceived by Khomeini and (he suggests) decisively influencing a large constituency of middle-class Iranians to march under Khomeini’s banner. (The “thanatos” on a national scale in the title of Afkhami’s book refers to this “suicide” or death wish of the educated middle class). I wrote that Afkhami could not argue that the National Front leaders could have effectively altered the course of the revolution while also maintaining that men in far more powerful positions could not have changed the course of events at all.

³⁵ Arjomand’s analysis-critique of other theories of revolution in the light of the Iranian

Once the central authority began to disintegrate, according to Arjomand, others who had a claim to authority could step into the breach. The religious leaders proved the most successful of the groups that might have fulfilled this role, in part because the shah had already destroyed or coopted their potential competitors (the landlords and the tribes). The religious leaders had little trouble in securing followers among three groups: the dispossessed, dislocated, and declassed urban dwellers, including the tens of thousands of young men and women university graduates who had only recently come from their villages to the major urban centers, and new urban dwellers created by chain migration from rural areas and small towns; the traditional bourgeoisie, the petty bourgeoisie engaged in the retail trade, and the craftsmen of the bazaar; and the new middle class of civil servants, school teachers, the intelligentsia, and white-collar workers from the service sector.

The declassed and uprooted Iranians were ripe for recruitment. The bazaar merchants, retailers, and craftsmen had genuine economic grievances and a class interest in seeing the shah overthrown. Arjomand devoted considerable space to explaining why the members of the new middle class, contrary to their apparent class interests, were willing to march under Khomeini's banner. Like Afkhami, Arjomand believed that the members of the new middle class deluded themselves into believing they could realize their own aspirations under clerical leadership. Largely because of policies pursued by the shah, the new middle class was atomized and lacked organization or leaders. Its ability to act was seriously impaired by the isolation of the army officers, who were too closely identified with the shah to serve as allies.

Furthermore, Arjomand argued, the members of the new middle class (like the declassed, uprooted urban dwellers) also suffered from a pervasive sense of moral disorder, disorientation, and anomie. They desired inclusion in political society; having lost their moral moorings, they welcomed the idea of the regenerative power of revolution. Besides, Arjomand noted, this would not be the first time classes participated vigorously in a revolution that did not further their interests and that victimized them. In the discussion of this sense of anomie and moral disorder, there is a blurring in Arjomand's analysis of the new middle class and the rootless, declassed Iranians of the great urban centers. If for Abrahamian, the revolution was made by the "modern" middle class and the industrial working class but was captured by the "traditional" middle class, for Arjomand, by contrast, the revolution was led and captured primarily by the declassed, who also provided it with its storm troopers.

Thus Arjomand draws heavily on the fascist political culture of Nazi Germany and Mussolini's Italy to explain certain features of the Iranian Revolution—the mobilization of rootless urbanites, mass politics, the emphasis on revitalization of the moral order combined with a strong emphasis on cultural purity and the rejection of "alien" ideologies, the relative unimportance of economic considerations, as well as a number of other elements. This fascist parallel is mentioned

Revolution and his attempt to offer an alternate theory can be found in *Turban for the Crown*, 189–210. The quotation, "mostly in the pits," is from 191.

only in passing by Keddie and figures little in the other analyses of the revolution covered in this essay.

Arjomand shares with Zonis and Abrahamian an inclination to give weight to the role of personality in explaining the revolution. Like Zonis, he believes that the shah could have deployed the army to confront the revolutionaries, and, along with both Zonis and Abrahamian, he believes the personality of Khomeini to be important both in the politicization of Shi'ism and in determining the course of the revolutionary movement.

Arjomand, like Zonis, resorts to a form of psychological analysis (or, from Arjomand's own perspective, to the application of Durkheimian categories) to account for the large number of members of the new middle class and declassed urbanites who flocked to Khomeini's cause. Zonis identifies one set of feelings ("narcissistic rage") and Arjomand another ("anomie," "disorientation") to explain this phenomenon. Arjomand's analysis, like Keddie's, emphasizes the specifically Iranian cultural characteristics of the revolutionary movement and its outcome. For example, Arjomand attributes Khomeini's charismatic appeal not only to his personality and qualities as an individual but also to concepts of leadership and the sanctity of religious personages rooted in Shi'i thought. His culturally specific analysis of the Iranian revolution is also evident in his emphasis on the role of Shi'ism, both in the years before the revolution and in the postrevolutionary period, although Arjomand would see his concluding chapter as an analysis of revolution that is general rather than culturally specific.

THESE VARIOUS EXPLANATIONS CONTAIN MUCH THAT IS WORTHWHILE, but they also highlight how much we still need to learn about the Iranian Revolution. Obviously, simply to point to lacunae in our knowledge (which applies to the nineteenth century as well) is a pointless exercise, for the gaps are considerable. But it is striking that much of what is written about the Iranian Revolution applies primarily to Tehran; virtually nothing is known about how the revolution played itself out in the provinces. In the protests that led to the overthrow of the monarchy, there was massive participation across a wide social spectrum. But, intelligent guesswork aside, little is known about the composition of the revolutionary crowds.

Few studies have been done on popular political culture. For example, although much has been written about Islamic reformist thinkers like Ali Shariati, whose influence among university students and other educated people was considerable, virtually no work has been done on the ideas propagated by preachers who worked among the urban working class, or sub-proletariat, or who preached in the popular mosques, in provincial centers and in the countryside.³⁶ We know little about the ideology that motivated the working-class followers of Khomeini, whether in the large modern factories, the small traditional workshops, or among the underpaid and unskilled shopboys of the bazaar. Women, particularly women

³⁶ For an analysis of some of the popular pamphleteering literature of the year preceding the revolution, see Shaul Bakhash, "Sermons, Revolutionary Pamphleteering and Mobilization: Iran, 1978," in Said Amir Arjomand, ed., *From Nationalism to Revolutionary Islam* (London, 1984), 176–94.

from working-class districts of Tehran, were major participants in the mass protests against the Pahlavi regime in 1978. But it is impossible to say whether they believed the revolution would bring them liberation; whether, as the clerics allege, the majority of women truly desired to revert to Islamic dress and traditional Islamic law on matters of marriage, divorce, child custody, and the right to work (and thus to give up all the advances in women's rights achieved under the previous regime); or did not think in these terms at all.

The Iranian Revolution clearly involved an uprising by the poorer and less privileged classes against the wealthier and more privileged members of society. But it is also possible that the *outcome* of the Iranian Revolution is better understood not in terms of class but as the seizure of the state by a new but narrow and self-serving ruling group. The social origins of this ruling group do not necessarily tell us much about the classes that dominate Iran in the postrevolution period or about those to whose benefit policies are determined. It may be true, as Abrahamian argues, that the petty bourgeoisie has largely remained immune from the property seizures and expropriations initiated by the revolutionary government. But all property is insecure in postrevolutionary Iran. Some distribution of wealth has taken place, but the transfer of wealth has been not so much from one class to another as from the private sector to that already swollen leviathan, the state. The primary beneficiaries are the powerful men of the state and their associates—merchants, middlemen, distributors, contractors—in the private sector. As has happened repeatedly in modern Iranian history, power is concentrated in the state and is ultimately arbitrary. What exists of civil society is to be found in the private space that small groups of people manage to carve out for themselves, away from the long arm and intrusive eyes and ears of the state.

Reviews of Books

GENERAL

LEON POMPA. *Human Nature and Historical Knowledge: Hume, Hegel, and Vico*. New York: Cambridge University Press. 1990. Pp. viii, 234. \$44.50.

This book is concerned with the problem of how historical knowledge is possible. Historical knowledge is not possible without an understanding of human nature. But this gives rise to the question of whether human nature may have changed in its history. Most theories of human nature are timeless and universal. Some are purely formal, specifying only limiting conditions, and so can provide no way of determining factual from fictional accounts of the past. Others have a content but impose a decision a priori about whether changes in human nature have occurred. What is needed is a conception of historical knowledge that leaves the historicity of human nature a factual question to be determined by historical criticism.

To this end, Leon Pompa's main thesis is that our knowledge of the past is not founded on argument, inference, or evidence. Rather, it is a set of inherited beliefs that have filtered down to us through the traditions that make social experience possible. Historical knowledge is socially acquired and is essential to our being in a historical world. In inheriting beliefs about the past, we also inherit beliefs about how our historical consciousness has changed. It is this inherited body of beliefs about our past and present that constitutes the genuinely historical content of human nature. Historical criticism (the use of documents and argument) goes on within this domain. Ruled out a priori is any conception of historical criticism that pretends to be independent of the historically transmitted domain of belief.

The greater part of Pompa's book is an attempt to show how the founding theorists of history—David Hume, Georg Wilhelm Friedrich Hegel, and Giambattista Vico—fail to frame a genuinely historical conception of historical knowledge. On Pompa's reading, Hume's doctrine of the uniformity of human nature denies any significant change in human nature. The opposite error is made by Hegel's doctrine that history is the unfolding of the idea of freedom and by Vico's doctrine of the "ideal eternal

history," whereby human nature necessarily passes through three stages. Both theories make it such that human nature must change in a certain way. So neither Hume, Hegel, nor Vico leave it a contingent matter as to whether human nature has changed.

Those interested in the human sciences will find it worthwhile to work through Pompa's carefully reasoned and insightful argument. Those who are interested in the thought of Hume, Hegel, and Vico on the problem of historical knowledge will be disappointed. These theorists appear mainly as vehicles for Pompa's argument. The treatment of Hume is especially off the mark. It shows little understanding of recent work on Hume as a philosopher of the human sciences. No mention is made of Hume's conception of history as revealed in the *History of England* and in the *Essays Moral, Political, and Literary*. Both works contain observations about radical changes in human affairs and how they can be understood. In the *Treatise* and in two *Enquiries*, Hume developed a social conception of knowledge, holding that social and political order is determined by opinion. The uniformity of human nature that Hume teaches is very like the uniformity that Pompa's own conception of historical knowledge requires, namely the psychological conditions necessary for the transmission over time of socially acquired knowledge. It is ironic that the conception of historical knowledge that most resembles his own is not Vico's, as he suggests, but Hume's.

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BRYAN D. PALMER. *Descent into Discourse: The Reification of Language and the Writing of Social History*. (Critical Perspectives on the Past.) Philadelphia: Temple University Press. 1990. Pp. xvii, 289. \$34.95.

This book, Bryan D. Palmer tells us, is an introduction to the ways in which historians, particularly social historians, have used literary theory and poststructuralist thinking, an approach that "posits the centrality of discourse or language in constructing being, power, and consciousness" (p. xii). The roots and implications of discourse theory are not well understood, he believes. Poststructuralism, he argues, has

downgraded historical materialism as an approach to social history, whereas class and class struggle should be central to our understanding of the past "as categories of analysis and essential components of lived experience" (p. xiii). Major contributors to the literature of class analysis recognized the importance of language in their work, but they also prepared the way for the adoption of literary theory. "Critical theory," however, "is no substitute for historical materialism; language is not life" (p. xiv).

The first chapter summarizes the positions of major contributors to linguistic theory beginning with Friedrich Nietzsche and Ferdinand de Saussure, touches on the work of important Russian scholars, notes the structuralist contributions of Claude Lévi-Strauss, and examines Roland Barthes's legacy. After glancing at Louis Althusser, Palmer moves on to Michel Foucault, Jacques Lacan, and Jacques Derrida, who "commanded an army that he was incapable of leading and controlling" (p. 40). Next he indicts Dominick LaCapra for misunderstanding the varied nature of social history and surveys the works of authors who approach history as historical materialists but address language "in ways that are instructive" (p. 86). Here enter Karl Marx, C. L. R. James, Ariel Dorfman, Raymond Williams, and E. P. Thompson, plus Joan Scott as Thompson's deconstructive critic. Her efforts are termed "rather flat," revealing "selective instancing" and "obvious misreading and overt distortion" (p. 85).

Palmer next moves to the realms of politics, class, and gender. In political history, he focuses on the French revolution and American republicanism, identifying François Furet and Lynn Hunt as militant linguistic analysts of the French revolution, whereas the approach spread almost surreptitiously in American republicanism. In considering class, Palmer targets Michael Kazin, Jacques Rancière, Gareth Stedman Jones, and William Reddy and then reaffirms the utility of class and class struggle as working concepts. He believes that "the case for discourse can be made with more force" in research on gender (p. xv). After noting national differences in approach, he introduces us to Robin Lakoff, Dale Spender, Julia Kristeva, Luce Irigaray, Judith Walkowitz, Myra Jehlen, Denise Riley, Jeffrey Weeks, and (once more) Joan Scott. He finds recent emphasis on the lingual interpretation of natural differences between the sexes to be oversimplified.

Palmer makes various major points in his conclusion. The influence of critical theory has not been all bad. Before it "reaches the point of the reification of language, it contains insights and guides capable of opening new . . . understanding" (p. 189). "Much writing" in this style, however, is "*crap*, a kind of academic wordplaying" (p. 199); more regrettably, "the privileged terrain of social history is no longer economically generated and politically situated relations and struggles, but the expression those struggles and relations take in culture, ideas, sexuality—in

discourses" (p. 203). We should, argues Palmer, "begin to reconsider the . . . neglected importance of determination" (p. 211) and save the best of both approaches.

Palmer identifies his audience as historians, particularly apprentices, and workers in related disciplines. Within this context this is an important book; it deals with significant questions and implicitly raises many more. Palmer skillfully guides the reader from important figure to important figure and is adept at turning a sulphurous or sardonic phrase. He himself refers to this book as a "polemic" (p. xviii), and readers may feel that his analysis is characterized by some "facile assertion" (a phrase applied to the work of Gerda Lerner) and undue selectivity (p. 146). Others may sense that they are witnessing an episode of clan warfare. And if my understanding of the level of sophistication among apprentice historians is correct, this book will produce some confused young scholars. Nevertheless, they will have learned from it.

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GAVIN I. LANGMUIR. *History, Religion, and Antisemitism*. (A Centennial Book.) Berkeley and Los Angeles: University of California Press, in association with the Center for Medieval and Renaissance Studies, Los Angeles. 1990. Pp. ix, 380. \$37.50.

This is an ambitious, stimulating, original work that emerged out of the author's longstanding investigation of medieval anti-Semitism and grew into a fresh analysis of the most basic concepts confronting historians of religion. Gavin I. Langmuir argues repeatedly that historians may not explain human actions simply as the result of religious beliefs. Not only must we know what motivated people to hold beliefs that we label religious but we must also develop a definition of religion itself that goes beyond the assertions of believers.

The author proceeds with an extensive, closely argued effort to redefine the key terms of discourse in the sociology and history of religion. "Religiosity" consists of "the salient patterns or structure according to which the individual human organism consciously correlates all the diverse processes occurring within the organism with those that surround and impinge on it in order to develop, maintain, and ensure the coherence and continuity of the distinctive elements of its identity" (p. 160). Although rational, empirical thinking plays some role in this process, religiosity is primarily a function of "nonrational thinking." "Religion" means "those elements of religiosity that are explicitly prescribed by people exercising authority over other people" (p. 136). This definition covers both traditional, "psychocentric" religions, and modern, "physiocentric" religions such as "dogmatic Marxism" and Nazism.

As for anti-Semitism, Langmuir alludes to his argument in the title essay of a companion volume, *Toward a Definition of Antisemitism* (1990), where he distinguishes between hostile but realistic assertions about outgroups, xenophobic assertions, and chimerical assertions, which "attribute with certitude to an outgroup and all its members characteristics that have never been empirically observed." If the term anti-Semitism should be used at all, he says, it should be confined to chimerical assertions about Jews, such as the blood libel, charges of host desecration and well poisoning, and Nazi racial theories.

To Langmuir, genuine anti-Semitism was born in the later Middle Ages and resulted from internal religious doubts among Christians that led to irrational rather than nonrational thinking as a mode of preserving essential beliefs. Jews, who reinforced these doubts, had to be depicted—ironically—as impervious to rational argument and capable of monstrous behavior. In modern times, irrational hatred of Jews also emerged in people who felt deeply insecure in their own, often untraditional religiosity. For Nazis, in fact, Jews had to be exterminated precisely because the empirical characteristics of real Jews were so manifestly incompatible with Nazi ideology that the tension could be relieved only by the physical elimination of the disturbing evidence.

Inevitably, a bold and revisionist thesis of such scope raises a host of questions. Despite frequent repetition and some brief argumentation, Langmuir never firmly establishes the basis for his refusal on principle to recognize the possibility that the content of religious beliefs could at least sometimes be the crucial explanation for certain actions. The line between rational and nonrational thinking is not always easy to draw, and in discussing such issues as the motives of medieval converts, his definition of religiosity leads him to underplay the role of rational argument. The evidence for his crucial assertion about the genesis of anti-Semitism in the religious insecurity of medieval Christians is extremely thin, and he does not sufficiently stress the possibility that even an explanation rooted in psychic insecurity might point to other than strictly religious forms of insecurity in the turbulent world of late medieval Europe. Indeed, the parallel discussion of modern times inevitably refers to inner tensions involving self-esteem and the role of the individual in society rather than traditional religious doubts. And, despite Langmuir's striking use of a famous quotation by Heinrich Himmler to establish his thesis about Nazi motivations, that thesis remains speculative in the extreme.

Finally, the definitions of key terms raise the question of how far such definitions can deviate from accepted usage before their usefulness is compromised. We have, for example, a definition of anti-Semitism in which Adolf Stoecker was not an anti-Semite; a definition of religiosity in which every human being is to a significant degree religious; and

a definition of religion that excludes faiths with a weak structure of authority, leads to the assertion that three Catholic religions (episcopal, royal, and papal) succeeded one another in the Middle Ages, and includes systems that reject any "supra-empirical reality." Although these assertions can all be defended, scholars will find it difficult to embrace terminology with so tenuous a relationship to ordinary discourse.

These questions are a function of the importance of this book, which will have to be read with care not only by Jewish historians but by anyone with a serious interest in the history or sociology of religion and prejudice.

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CAMILLE PAGLIA. *Sexual Personae: Art and Decadence from Nefertiti to Emily Dickinson*. New Haven: Yale University Press. 1990. Pp. xiv, 718. \$35.00.

Sexual personae such as the *femme fatale* and the beautiful boy exert a deep forbidden appeal. But what are we to make of them? Camille Paglia notes that "Feminism dismisses the *femme fatale* as a cartoon and libel" (p. 13). This interpretation was evident, for example, in Bram Dijkstra's well-received art history study, *Idols of Perversity: Fantasies of Feminine Evil in Fin-de-Siècle Culture* (1986), which upbraided nineteenth-century artists for their unliberated views toward women.

By contrast, Paglia takes the controversial position that there is an important "element of truth in sexual stereotypes." Sex is a brutal demonic force, she insists, and her analysis of literature and art from antiquity to the *fin de siècle* focuses on the violence and pornography that she believes have been "ignored or glossed over by most academic critics" (p. xiii).

Certainly, most academics have taken a dim view of Paglia's work, and there is no doubt that this is a bizarre and extremely personal book (the word "I" occurs constantly) without any of the standard academic apparatus. Her emphasis on the Marquis de Sade and Sir James Frazer makes for an odd theoretical framework. What is most controversial, however, is not Paglia's literary analysis, per se. She makes, for example, interesting observations about Gautier's "Mademoiselle de Maupin," Balzac's "The Girl with the Golden Eyes," and the poems of Emily Dickinson, although she obviously owes a great debt to Mario Praz, whose book *The Romantic Agony* (1935) she cites with respect. The book's notoriety stems from Paglia's larger argument, that the identification of woman and nature is not myth but reality. Biology is destiny, she maintains, inasmuch as only women are mothers, powerful and uncanny figures against whom men bonded together and "invented culture." According to Paglia, "The very language and logic modern

woman uses to assail patriarchal culture were the invention of men" (p. 9).

Not only does she contemptuously dismiss the feminist fantasy of a peaceful prehistoric matriarchy but she also takes a deliberately polemical stance and argues that "If civilization had been left in female hands, we would still be living in grass huts" (p. 38). Conversely, "There is no female Mozart because there is no female Jack the Ripper" (p. 247).

Paglia has been attacked as an academic conservative, in league with Allan Bloom and other defenders of the "Western canon," but no conservative would be so explicitly approving of pornography, homosexuality, and rock-and-roll. Bloom may despise Mick Jagger, but Paglia loves Madonna, and in a sequel to this book she promises to explore so-called "pagan" influences on contemporary popular culture.

Many readers will hate or dismiss *Sexual Personae*, but there is also much here that is fascinating. The book's appeal comes not so much from the logic of its arguments as from the play of Paglia's imagination as it concerns the powerful and dangerous erotic forces that lie at the base of human culture. Paglia does not contend that women are inferior to men (and she would be the first to use herself as an example of a brilliant female intellectual), but she does argue that biology limits eroticism, our imaginative perceptions of sexuality.

When artists and writers deal with erotic themes, they enter an often murky realm. As the controversy about Robert Mapplethorpe's photographs indicates, wishful thinking cannot limit sex to a wholesome phenomenon that modern decadents refer to as "white bread."

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LONDA SCHIEBINGER. *The Mind Has No Sex? Women in the Origins of Modern Science*. Cambridge: Harvard University Press. 1989. Pp. xi. 355. \$29.50.

Quest[i]one: *Foemina non est homo* (Question: Females are not human) begins the title of a German medical dissertation from 1688, stating the question of women's humanity that has long been at the center of gender politics in modern Western culture. There is no better starting place to understand the history, implications, and problems of that question than this elegant history of women's participation in the scientific debates that have shaped our definitions of society and of the human. Londa Schiebinger's book answers three questions: What possibilities and limits does gender set for women in science in the modern Western world? How did this gendered science come about in the new scientific centers of post-Newtonian Europe? How can we read the iconography of science for clues to this history?

Schiebinger chooses her central narrative carefully

and names it precisely in her title. She argues that from François Poullain de la Barre in 1673 to the present, the notion that "the mind has no sex" has had a real if unstable appeal to natural philosophers and their patrons as a banner of progressive modernization of the sexless soul. Another banner prevailed, however, one that categorically separated the "feminine" from science in the name of "gender complementarity," a theory of gender relations and professional science more crudely described in the nineteenth century as "separate spheres."

Using a remarkable array of published works (from obscure seventeenth- and eighteenth-century French and German journals to contemporary documents of the postmodern debate), Schiebinger examines the modern institutions and communities that gradually coalesced around the "new philosophy" of experimental and mechanical science. In chapter 1, she follows women from their utter exclusion by the monasteries and universities of medieval Europe to their placement on the "periphery" of scientific academies. Although Margaret Cavendish never made it into the Royal Society in spite of her contributions to research, Schiebinger reveals that an entire class of women of "rank and patronage" had a place at the conversational table as full participants. Some self-confident souls even thought that women might be especially suited to this natural philosophy. In chapter 2, we hear Gottfried Wilhelm Leibnitz writing to his patron and colleague, Queen Sophie Charlotte: "I have often thought that women of elevated mind advance knowledge more properly than do men. Men, taken up by their affairs, often care no more than necessary about knowledge; women, whose condition puts them above troublesome and laborious cares, are more detached and therefore more capable of contemplating the good and beautiful" (p. 39).

To this story of rank and patronage Schiebinger adds the message of gender or, one might say, the genders of several classes and locales. Experimental science honored the work of craftswomen in Germany. The astronomer Maria Winkelmann worked alongside her husband as full participant, even though that work did not provide her with membership in the Berlin academy after his death. This recovery of "lost women" is also the recovery of lost classes of scientific workers, and of the participation/stigma duality characteristic of science before it became a profession.

Schiebinger provides a detailed social history of science when it was both diverse and unstable, but uncovers only a glimpse of its content. In the chapter on "the denigration of women's traditions," for example, we learn that as medical recipes were dropped from cookbooks so also did women lose their status as healers. But we do not connect this story with the topics men and women discussed at the conversation table or in the observatory. We need some cognitive guidelines as we move from the cookbooks and the conversations of early modern society to the language

of this new man, the "scientist," a general term coined by William Whewell in 1834, in the same paper in which he "assured his readers that 'notwithstanding all the dreams of theorists, there is a sex in minds'" (p. 1).

Whewell's conclusion came, however, only after a long period of "battles over scholarly style" and an iconology capable of "locating sex and gender in the natural order" (p. 160). In her most significant chapters, she first explores a world we have lost in science, where the image of science and the grammatical gender of the texts informed the reader of the nature of Scientia and hence its value. In the long history of this cultural struggle (which the author delineates employing scholarship from Plato to Antoine-Laurent Lavoisier), Nature appears consistently as female, the scientist usually as male. But science itself, also an icon in the early modern world, was sometimes male and sometimes female, an ambiguous sexuality that feminists welcomed in the name of their claim to humanity. They lost and, by the end of the eighteenth century, the masculinity of science was established. A woman scientist, Immanuel Kant opined, "might just as well have a beard." "Kant's attack on the feminine icon," Schiebinger says, "signaled a turning point in the historiography of science. The passing of the feminine icon also marked the passing of the classical appreciation of women's contributions to the sciences" (p. 146).

In the next chapter Schiebinger correlates the departures of the feminine icon and woman scientist with the rise of the female skeleton, a sexualization of the object of scientific study. This chapter adds to her work published in *Representations* in 1986 with a suggestive (but unrealized) portrait of sex and race (as in the German *Geschlecht*) as inseparable components of a world in which to be white and male was to have endowment of superiority.

Where did this construction leave women? Kant voiced the new stereotype of complementary sexes. "The fair sex can leave Descartes' vortices to whirl forever without troubling itself about them. . . . Her philosophy is not to reason, but to sense" (p. 271). Trivial but powerful, this equation of the sexed female with the feminine, subhuman mind was irresistible to those interested in distancing women from the conversation table. But why? The book should at least refer to some of the colonizing interests, sexual anxieties, and economic concerns of the capitalist West as it moved toward the establishment of a scientific profession while stabilizing its predilection for the masculine, and men, in science.

Schiebinger raises a number of interesting questions but leaves them unanswered when she turns from the female to the "feminine" and to the philosophical problem of gender and science for feminism. In the concluding paragraph, she remarks that "'femininity' represents a consistent set of values excluded from modern science" (p. 277). The feminine in most of this book, however, is not a set of values but a set of

behaviors (often symbolic) by and toward women. And it is a story of instability as much as consistency. Schiebinger's documenting of symbolic behaviors and unstable traditions in gender and science, however, is what gives her book its brilliance and originality.

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LUCILLE B. RITVO. *Darwin's Influence on Freud: A Tale of Two Sciences*. New Haven: Yale University Press. 1990. Pp. xii, 267. \$30.00.

Lucille B. Ritvo's work pertains directly to two massive and still rapidly growing bodies of scholarship, that on Darwin and that on Freud. This book in addition has elements that should appeal to general intellectual historians, for the author has set about re-creating part of the intellectual milieu in which not only Freud but many other major thinkers matured. Freud, she shows, provides an excellent as well as important example of what biology taught the Victorian generations and particularly exemplified how Darwin's influence could work itself out.

The volume is an expansion of Ritvo's classic article of 1965 on Freud's Lamarckianism and other work in which she called attention to the vital role of Freud's teacher, zoologist Carl Claus, in transmitting Darwinian ideas. In this book she also emphasizes an influential work by Darwinist Fritz J. F. Müller (*Für Darwin* [1864]). Ritvo's approach is primarily analytic, and most of her chapters systematically explore various themes in Darwin's writings that showed up, or seemed to show up, in Freud's writings. Ritvo does indulge in full argument and permits herself a Whiggish passage here and there. She has also decided, apparently, to rely on primary sources, including pre-World War I comments on the history of biology, with only a minimum of material from the recent detailed scholarship on Darwin, excluding even the work, for example, of William Montgomery; Freud scholarship, by contrast, she has mined fully.

Ritvo does not insist on any specific or dramatic influences but, by combining large amounts of circumstantial evidence, builds a strong case that young Freud drew from a particular body of biological knowledge of the late nineteenth century and assumed that his readers knew about and accepted the world picture that the German Darwinists developed. "No claim can be made for Darwin as the *exclusive* source of any one of Freud's ideas," she notes (p. 189); "Darwin's achievement was the convincing synthesis of an enormous quantity of essential observations that threw new light on *old* ideas." The themes that Ritvo explores therefore provide a context in many ways broader and richer than the reductionist and mechanistic aspects of Freudian theory explored by Rainer Spehlmann (*Sigmund Freuds neurologische Schriften* [1953]) and Peter Amacher (*Freud's Neurological Education and Its Influence on Psychoanalytic*

Theory [1965]), and more broadly but schematically by Frank Sulloway (*Freud, Biologist of the Mind* [1979]), who focused on anatomy and physiology.

Ritvo writes primarily about patterns of thinking to which Freud was exposed. One was a genetic point of view that was not Aristotelian but, by emphasizing use (or experience), provided a basic orientation for all of Freud's writing. As Darwin and Claus reintroduced the idea of recapitulation, it, together with the idea of acquired characteristics, ordered a large number of facts for Freud and for many others of his generation. Still another main theme is the idea of adaptation and conflict, with conflict so prominent that Freud to the end had no feel for stasis or balance such as Claude Bernard introduced, and a version of which showed up in later psychoanalytic thought in the form of conflict-free ego elements.

From actual work in biology, Freud acquired other habitual ways of thinking: the idea of gradation so that types of functioning did not have clear divisions but merged into one another (as did the ego and id); the idea that in one species there could be two different forms at the same stage of development, both of which are "normal"; and of course the idea that retrogression is possible and that, even beyond theoretical recapitulation, earlier developmental stages can leave traces on the later organism. With the coming of Darwin, such flaws in nature became not degradation or depravity but natural phenomena, to be treated as such.

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YIRMIYAHU YOVEL. *Spinoza and Other Heretics*. Volume 1, *The Marrano of Reason*; volume 2, *The Adventures of Immanence*. Princeton: Princeton University Press. 1989. Pp. xiii, 244; xv, 225. \$45.00 the set.

Volume 1 of Yirmiyahu Yovel's work establishes the background against which a better understanding of Spinoza's philosophy may be gained; volume 2 traces the fortunes of Spinoza's thought—its spirit rather than its letter—in nineteenth- and twentieth-century thought. These are relatively big subjects, treated too thinly to warrant two overly expensive volumes.

The second volume lays little claim to being a history. It seeks to demonstrate the ways in which certain luminaries of modern thought (Kant, Hegel, Marx, Nietzsche, and Freud) worked out their direct or, too often, indirect encounter with Spinoza's philosophy of immanence. The association of these great names rather than hard and fast evidence of a persistent confrontation with Spinozan ideas (Yovel, for good reason, often prefers the murkier phrase "climate of ideas") cannot but inflate Spinoza's contribution to the making of the modern mind—intentionally to inflate the significance of this enterprise? Contrary to Yovel's assertions, this volume does not in

any significant fashion give us a new perspective on the evolution of modern philosophy.

In volume 1, Yovel insistently and all too repetitively argues that Spinoza's philosophy can be properly understood only from the historical perspective of his Marrano background. (Marranos were forcibly converted Jews who in various forms and to varying degrees continued to adhere to their ancestral religion or "nation" and who were thus forced by endless stratagems to live a double life of religious dissimulation.) There is in Yovel's elegant and moving description of the "larger psychocultural milieu of Marranism" much that is sensible and much that is fanciful. Far too much of it, unfortunately, is based on intuitions rather than on a careful examination of the literature or, least of all, on a wide perusal of inquisitorial documents (our primary source in Marrano matters). Ostensibly interested in highlighting especially the "existential" dimensions of Marranism, Yovel never bothered to read what Marranos themselves had to say about their double lives of "triple alienation" (Yovel's characterization). Especially when dealing with a phenomenon as secretive and "existential" as Marranism, it is imperative to listen most carefully to the voices of the victims themselves. Or one condemns oneself, at best, to highly imaginative and graceful formulations of a basically ahistorical, almost tautological, nature. Yovel emphasizes in particular two basic Marrano traits: their habits of equivocation and their search for salvation outside the established church. Neither one, in sixteenth- or seventeenth-century Spain, was necessarily restricted to the Marrano community, unless, of course, as Yovel's mentors in things Spanish seem to favor, one automatically declares every manifestation of these traits in itself a strong indication of Marranism. These traits are then elevated to "psychocultural" status and are thus presented as having constituted the foundations of Spinoza's philosophizing, notwithstanding the fact that Spinoza had never been a Marrano but had been born a Jew, had never dissimulated in matters of religion but had accepted his own excommunication without contradiction or hesitation. Stressing the elements of equivocation and the search for alternate ways to salvation, Yovel presents us with an interesting reading of Spinoza's writings, with a philosopher of secular salvation through knowledge who employed various techniques of equivocations to communicate his ideas simultaneously to the "multitude" and to the philosophically sophisticated. I very much doubt that Yovel's interpretation will stand the test of time.

Many of the distinct insights mentioned by Yovel are not new; some are acknowledged as such, others not; many also were on their way out. In the absence of much research into the primary sources, Yovel's indebtedness to the authors of secondary works (whether Israel Révah, Américo Castro, Stephen Gilman, or Leo Strauss) is far greater than the sparse notes (and the obligatory criticisms) would ever lead

one to believe. In the present context—a context already far too laden with equivocations—this is not a minor matter. The same discomfort of not knowing where the source ends and Yovel's speculation begins haunts this entire volume. When all is said and done, even were we to make allowances for imaginative speculations of a psychocultural nature, ahistorical perceptions, and the niggardliness of acknowledgments, the ultimate result is a volume that adds disappointingly little to our understanding either of the Marrano phenomenon or of Spinoza.

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DONALD R. KELLEY. *The Human Measure: Social Thought in the Western Legal Tradition*. Cambridge: Harvard University Press. 1990. Pp. xiv, 358. \$35.00.

During the later fifth century B.C., many Greek intellectuals participated in what is today called the *nomos-physis* controversy. *Physis* is roughly translated as "nature," but sometimes as "reality." *Nomos*, more difficult to translate, means either "convention" or "statute" (convention enacted into law). The controversy arose in relation to ethics and politics, and it came down to this: if artificially contrived *nomos* conflicts with natural *physis*, which should prevail because it is "just," and which does in fact prevail?

The controversy influenced later philosophy. In the *Nicomachean Ethics* (5.7), Aristotle distinguishes "Natural Justice" (*physikon dikaion*), which is universally "just" without regard to individual human belief, from "Conventional Justice" (*nomikon dikaion*), which is "just" because of local acceptance or enactment—for instance, standards of measure used in markets.

Much of Donald R. Kelley's book examines the role this distinction has played in Western legal history, from Greece and Rome to modern times. The theme is vast, perhaps too vast for a single book. But Kelley's central point is clear: despite the perennial attraction of universal standards of justice (however these standards may be discovered or revealed), the strength of the Western legal tradition results from its receptivity to more local and "human" standards of justice. In what Kelley reconstructs as a battle between *nomos* and *physis*, *nomos* regularly wins. As the book concludes, "in this human world, for good or for ill, however modified by demography, technology, genetics, and psychotherapy, and whatever we may suppose or pretend, King *Nomos* still rules" (p. 283).

Unfortunately, as the sentence just quoted suggests, much the same is also true for Kelley's own book. Kelley's exposition of the "grand tradition" of Western jurisprudence is rich in detail, much of it gripping, particularly the chapters on medieval Germanic law, French customary law in the early modern period, and the German "Historical School" of the

early nineteenth century. All this repays close reading.

The broader argument of Kelley's book, however, is more suspect. Like Giambattista Vico before him, Kelley construes the Western legal tradition (and its progeny in the social sciences) as a potential challenger to post-Cartesian sciences. As he writes, "Nature and law, *physis* and *nomos*, these twin concepts, sibling rivals for the legacy of human wisdom, have interacted with each other across some twenty-four centuries of Western intellectual tradition; and indeed, in modern versions of the old contest, they are still interacting" (p. 1). As an example of the modern struggle, Kelley cites Richard Rorty's distinction between pragmatists "who work within small horizons and the limits of the human condition," and realists whose thought is "natural and not local." Kelley wants to tell "the story of the first community, the representatives of practical philosophy, if not of the 'pragmatist' canon in Rorty's sense" (p. 276).

Although Kelley frequently reaffirms this stance, it is difficult to regard his book as a useful contribution on the more abstract level. To be sure, although modern lawyers usually regard natural justice and law only as quaint covering words that once served to embrace a jumble of accepted social values, many earlier Western jurists took both concepts much more seriously, as affording an opportunity to rationally deduce and describe universally binding ethical and legal norms.

But even if the actual preeminence of *nomos* within the Western legal tradition can in fact be established, this is of general intellectual interest only to the extent that more local conceptions of law make any coherent contribution to understanding "the human condition." Here Kelley's book is unsatisfactory. In fact, the major contribution he ascribes to the upholders of *nomos* rests on an astonishing mistake he has made in interpreting a major source.

Kelley outlines the chief feature of this contribution on pages 8–10: a "trichotomy" described as corresponding "to the tripartite structure of civil law from the beginning down to the present." The trichotomy comprises, first, the individual "person" possessed of consciousness; second, "physical 'reality,'" referring "not merely to objective nature but to the material means of subsistence"; and third, "'action'—fundamentally social interaction among persons in the context of economic reality." So, Kelley argues, "the epistemological foundation of *Nomos* . . . is subject-centered and in effect expands concentrically from the center through ever-widening actions and experiences accomplished in a complex social field defined by cultural (and eventually legal) boundaries."

It is unclear that this trichotomy would actually be a major contribution to social thought (it seems too pedestrian for such a role), nor do the chapters that follow shed much light on the matter. In any case, Kelley is wrong to argue that the trichotomy corre-

sponds to a traditional tripartite structure of Western civil law.

On pages 48–52, Kelley discusses the organization of Gaius's *Institutes* (mid-second century A.D.). Near the outset of this elementary textbook, Gaius states (1.8): "All the law we use pertains either to persons or to things or to actions." Kelley glosses: "In other words, the social field was distributed exhaustively into the categories of 'personality,' 'reality' . . . and the actions and interactions of persons and things, including commerce as well as conflicts over property." This is, says Kelley, the "fundamental threefold structure which permeated all of Roman (and much of modern) social and legal thinking": "the old Gaian trinity" (p. 199), "the celebrated Gaian trichotomy" (p. 116), "the inevitable Gaian triad" (p. 167).

Nothing of the sort is true. In the first place, Gaius is describing "all the law," not the "social field" to which law applies. "Persons" (*personae*) may correspond to "personality," though family law is also embraced. But by "things" (*res*) Gaius designates not just property but also how property is acquired; thus, his middle books treat both property law and the laws of succession and obligation, including contract and delict—in short, all substantive law apart from "persons." By contrast, "actions" (*actiones*), the subject of the fourth book, are not "the actions and interactions of persons and things" but rather judicial actions: the law of procedure. Kelley, although dimly realizing the misinterpretation he thrusts on Gaius (p. 51), never comes to grips with it.

From a legal perspective, Gaius's actual trichotomy, which accords due weight to procedural law, is far more sensible than Kelley's. More important, the Gaian trichotomy is picked up by Justinian's *Institutes* (1.2.12) and *Digest* (1.5.1); later jurists, particularly in the mainstream "civilian" tradition, often adopted it. Kelley, although sensing the discord with his own theories (for example, pp. 117, 167, 214), avoids discussion. It is simply unclear that any Western jurist has ever adopted a "fundamental" trichotomy remotely resembling Kelley's.

Especially in the earlier chapters on classical and medieval law, Kelley makes numerous smaller and larger errors of fact; the material, although central to his thesis, seems unfamiliar. But no error is so significant as the one just described, which vitiates the book's entire argument. In any case, surely it is doubtful that practical thinkers, working within a purely historical horizon, can ever hope to offer more than local insight into "the human condition." Indeed, for most lawyers, local insight more than suffices. In this respect, legal theory affords limited purchase for "a Copernican Counter-Revolution, in which the 'person' is indeed, in terms of experience, at the center of the cosmos" (p. 282).

Once Kelley's theoretical superstructure is stripped away what remains is a more modest, but also a much more successful book. If *nomos* teaches anything, it is

this: regard grand theory skeptically, and keep horizons small.

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CHARLES TILLY. *Coercion, Capital, and European States, AD 990–1990*. (Studies in Social Discontinuity.) Cambridge, Mass.: Basil Blackwell. 1990. Pp. xi, 269. \$34.95.

Charles Tilly's medium-sized book about large-scale political change in Europe and the world makes a number of huge arguments, among them: that the formation of the national state in the last few centuries—a centralized form of territorial rule that the author distinguishes from the nation-state, an ideal of cultural unity that not even Great Britain, Germany, or France approximated (p. 3)—was an unintended side effect of making war; that warmaking led states in Europe from a condition of diversity and difference (especially after 1490, when more than five hundred states persisted, including cities, city-states, networks, federations, leagues, and empires) to converge on the national state (the thirty or so states in the nineteenth and twentieth centuries); that within this convergence there remained diversity of state structure and strength, the result of a differential ability to finance war through a combination of coercion and capital accumulation (hence the role of cities as commercial centers); that the developing national states of Europe after 1490 took shape within a developing state system further shaped by the demands and shifting character of war; and that the geographic and temporal variations of state formation in Europe and the domination of European states in the world were the outcome of both specific historical circumstances and a general distribution of both coercion and capital. Tilly asks big questions, and he has big answers. But, in "pursuing a history that oscillates between the somewhat particular and the extremely general" (p. 16), he risks reducing the global to the glib and occasionally falls victim to writing "a cartoon history of Europe [between 990 and 1990] in four panels" (p. 206).

"Reviewers will no doubt waste many of their words trying to pigeonhole the book into one school or another," writes Tilly (p. 34), as he usefully outlines alternative schemes ranging from world-systems theory to political modernization frameworks to models of capitalist development. Borrowing eclectically, especially from the work of Barrington Moore and Lewis Mumford, he favors an approach in which war and its financing is central, and that transcends his earlier attempts, with Stein Rokkan and others in *The Formation of National States in Western Europe* (1975), to develop a single-path model of state-building. But, after demonstrating the movement from diversity

toward unity within Europe, Tilly ends with something like a single model in a final chapter on the military rule of governments in the non-Western world during the twentieth century. These states suffered a counterexperience to European "civilianization" of rule after 1850; their failure to develop forms of "direct rule" with significant consent or support of citizens led to military dictatorships. Tilly rightly worries about the "intellectual colonialism" implicit in this argument as he argues forcefully against assumptions that military power in the non-Western world is a "natural phase of state formation" (p. 224).

This work is typically Tilly: intelligent, erudite (with a vast and useful bibliography), and full of hubris. He claims to account for the particular historical experiences of European national states—the "empirical regularities" of historical record. But, despite his flair for the telling detail and the illuminating example, historians of the distinct periods and states through which he moves will undoubtedly react with shock to some of his more broad and speculative claims as well as to some of his misleading assumptions (for example, that "the end of the Hundred Years' War freed a relatively unified France to look around for spaces to conquer" [p. 76]). Tilly is the first to admit that he "deal[s] with historical facts like a rock skipping water" and offers a variety of tests that could be used to refute his claims (pp. 35–36). But to accept the challenge would require a debate not only over the "empirical regularities" of record but also his categories of interpretation. Without adopting the entirety of his language or arguments, political, economic, and military historians—perhaps especially those of early modern Europe—may find in this book useful illuminations of their own corners of the world. Although they occasionally may be swept away in the global historical movements, they are likely to be disappointed by the absence of local historical agents at work.

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ANTHONY PAGDEN. *Spanish Imperialism and the Political Imagination: Studies in European and Spanish-American Social and Political Theory, 1513–1830*. New Haven: Yale University Press. 1990. Pp. viii, 184. \$22.50.

To a reviewer whose specialty is not Spanish, Italian, or Latin American history so much as the history of political discourse and consciousness, the value of this book is plain. History of political discourse needs to become more cosmopolitan, to acquire a wider geographic range, to study and compare the various images and problems that the mind has discovered in the political experience of many cultures—within the global range of Western civilization, as well as in other civilizations making up world history. Anthony Pag-

den, already the editor or coeditor of volumes on *The Languages of Political Theory in Early-Modern Europe* (1987) and, with Nicholas Canny, *Colonial Identity in the Atlantic World, 1500–1800* (1987), surveys in this monograph responses to the Spanish empire by those living within it, though not by those governing or possessing it. What Castilians or other Spaniards understood their empire to be does not much appear, apart from a summary of the well-known debate among jurists and theologians from Vitoria to Sepúlveda (which Pagden has memorably examined in *The Fall of Natural Man* [2d ed., 1986]). We hear instead from the Calabrian Tommaso Campanella, the Neapolitans Paolo Mattia Doria and Antonio Genovesi, the Mexicans Carlos de Sigüenza y Gongora and Francisco Xavier Clavigero, the Peruvian Juan Pablo Viscardo, and the Venezuelan and frustrated American hero Simon Bolívar; and what we seem to be reading is a series of studies in the pre-enlightened and enlightened theory of civil society, showing how a succession of very able minds sought to discover the origins of political legitimacy that their societies might have possessed before they were incorporated within the Spanish empire, and that they might use in asserting their autonomy within or outside it.

To an Anglophone or North American reader, two broad comparisons may occur. The Spanish theory of empire was more coherent than the British; it defined an empire as a number of provincial kingdoms or viceroyalties held together by an imperial crown, a theory precluded by the English determination not to be confederated with Scotland. Within this structure, the problem facing Naples or Peru was that of resisting the hegemony of officials or interlopers from the central kingdom of Castile; but in confronting this problem, each outer kingdom was required to define or redefine its civil structure and its antedating and legitimizing history. British Americans found this relatively easy: they claimed first that they shared the history and the rights of the English, and second that they were entitled to make a revolution in the name of the universal rights and virtues already latent in that history. But Castile did not provide a myth of Spanish liberties which Neapolitans or *criollos* might universalize, and intellectuals must seek legitimacy and liberty in a *jus gentium* or a theory of social history to which Spain made no particular contribution after those of the great jurists of the sixteenth century.

Here the Neapolitan and American stories are seen to diverge. After the utopian and neoghibelline universal monarchy imagined by Campanella (which Pagden presents as something of a false start), Doria and Genovesi accused Spanish rule of failing to integrate the nobilities of the *regno di Napoli* according to the humanist vision of service to the *repubblica*, leaving them to practice a false and exploitative ideal of their own *onore*. There comes into view the tragedy of the modern south of Italy, in which the network of

private understandings is as strong as the state but rather more criminal. In Spanish America, the governing yet passive presence is that of the huge majority of pre-European indigenes, and Pagden shows interestingly how Aztec and Inca history was used—as that of the Iroquois was not—to affirm the existence of imperial rights that *criollo* settlers might inherit. Here too, however, civil humanism fails, and Bolívar dies asserting that only the heroic impossibility of a republic of virtue can make his Americans capable of an ordered polity. Commerce and virtue have failed in their debate, and the enlightened continuation of humanism cannot produce even a Jacobin quasi solution. Pagden provides a richly informative but not an encouraging contribution to the history of political consciousness.

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BURTON STEIN. *Thomas Munro: The Origins of the Colonial State and His Vision of Empire*. New York: Oxford University Press. 1989. Pp. vi, 374. \$32.50.

"The ruling vice of our government is innovation," Thomas Munro wrote after more than forty years as an administrator in India, "and innovation has been so little guided by knowledge of the people, that it must seem to them little better than caprice" (p. 292). Munro was voicing the unease felt by many of the first generation of the new rulers as they brought about structural changes in Indian society, and Burton Stein has provided his readers with a detailed examination of the actual process of imperialism in a particular time and place. He does this by following Munro's career as a soldier and administrator in India and as an influential participant in London in the period around 1813, when the East India Company was being transformed from a trading corporation into the government of a vast empire. He builds on his previous studies of the social and political life of South India before the advent of the British and makes use of previously unavailable materials.

Stein gives careful attention to Munro's role in the military conquest of South India and in the institution of a new judicial system, but it is Stein's description of Munro's creation of a system of land tenure in South India, against great opposition from officials both in Great Britain and India, and Munro's "vision of empire," his justification for British rule in India, that inform much of the study.

As administrator of the "Ceded Districts," a vast area in South India half the size of England, Munro rejected the land tenure system that had been established in Bengal, which recognized a class of landlords (the zamindars) as owners of the land. Instead, he abolished all intermediaries between the government and peasants, the "ryots," and instituted what became known as the "ryotwari system." Whether the peasants under the new system were better off than

the peasants in Bengal, or than they were before the British came, has been much debated, but there is no doubt that it brought about radical changes in the social and economic fabric of South Indian life.

As for his vision of empire, Munro recognized that in many aspects of civilization the Indians were in no way "inferior" to the West: they had excellent manufacturing and agricultural skills, they had a good system for educating their children, and they were exemplary in their treatment of women. What they lacked, and what the West possessed, he argued, was the theory and practice of good government. This was to be provided, however, not by the introduction of unsuitable models from the West, such as a free press and elected officials, but through rule by autonomous British proconsuls such as himself, who were, as Stein puts it, "trained by a lifetime of service, knowledgeable of and sympathetic towards its cultures, languages, and peoples" (p. 208). India, ruled in this fashion, was to be a permanent possession, Munro said in 1824, until "the natives in some future age . . . become sufficiently enlightened to form a regular government for themselves" (p. 293).

Stein's achievement is to have explicated, with great clarity, this imperial vision in the context of the early nineteenth-century economic and political realities of both India and Great Britain.

AINSLIE T. EMBREE
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GEORGES MINOIS. *History of Old Age: From Antiquity to the Renaissance*. Translated by SARAH HANBURY TENISON. Chicago: University of Chicago Press. 1989. Pp. viii, 343. \$29.95.

Simone de Beauvoir began *La Vieillesse* (1970) by claiming, "It is impossible to write a history of old age," for literary sources "never say anything but part truths and very often they lie." That did not stop her from writing her book, and Georges Minois, in this translation of his 1987 survey of old age from antiquity to the Renaissance, responds, "Poets do not in fact lie; they see the world through distorting glasses created by their sensitivity, their social milieu and their education" (p. 48). Minois provides a more sensible reading of literary and demographic sources and benefits from some of the historical works published in the 1970s and early 1980s. Whereas Beauvoir found old age an unmitigated disaster, he finds a more ambiguous degradation and describes the historical pattern as "not so much a continuous decline as a switchback evolution" (p. 7).

Minois comments on the supposed reverence shown the aged in "primitive" or "barbarian" conditions (pp. 8, 133–34), but this book is essentially a reflection on representations of old age in Western civilization. His reading of the Old Testament suggests a shift from patriarchy to suspicion of the aged, which continues in the early stages of Christianity. In

classical Greece and Rome he finds that the elderly serve as an implicit critique of the idea of human perfectibility. The idealized statues of the early period have no wrinkles, and the positive image of old age in Plato's *Republic* is downright utopian. A more "realistic" depiction emerges in the Hellenistic period, borrowing from Aristotle's nastier view. Comedy and satire are merciless, as they would be in Rome and early modern Europe. Yet, in the Roman Republic, the elderly dominated the Senate and benefited from the *patria potestas*. But the Empire was less kind, and patriarchal authority lessened over the centuries. Minois describes the Roman dilemma well: "the more powers and power the law confers on them, the more they are detested by subsequent generations . . . Conversely, the more they are deprived of rights, the more they are despised" (pp. 82–83). He advises that one take Cicero's *De Senectute* with a grain of salt (pp. 105–12).

In the Middle Ages the aged were relatively silent, although not as absent as Jacques Le Goff has claimed (pp. 206–07). If only old warriors mattered in early medieval society, then the Church eventually provided monastic alternatives, which might have served as models for retirement homes. Still, medieval culture tended to use old age as a symbol, not to recognize old persons per se. But with the growth of late medieval cities, a greater variety of activities emerged, including retirement for a significant minority. The Black Death strengthened the position of the elderly, creating veritable gerontocracies. New and powerful roles gave rise to new and powerful resentments (pp. 209–48). Thus, in the Renaissance, which witnessed the emergence of many notable old people, Minois sees a revival of classical gerontophobia. He stops short of the Reformation, leaving the modern and contemporary period to Jean-Pierre Bois in a subsequent volume.

As in the work of Philippe Ariès, the general argument is overly schematic, and the book often reads like a Western civilization source book. The sources, however, are rich and interpreted skillfully. Although Minois has benefited from some important recent scholarship, the book is still a premature synthesis. New books, either just out or announced, will place the field on a less-impressionistic basis. The French paperback was important as a work of *haute vulgarisation*, and this translation will be of great interest to general readers, but it deserved more careful editing. Moreover, nowhere on the cover are the chronological limits of the book identified, and the eighteenth-century jacket illustration—the French cover reproduced Ghirlandaio's old man and young boy—is misleading.

DAVID G. TROYANSKY
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STANISLAV ANDRESKI. *Syphilis, Puritanism, and Witch Hunts: Historical Explanations in the Light of Medicine*

and *Psychoanalysis with a Forecast about Aids*. New York: St. Martin's. 1989. Pp. vii, 227. \$39.95.

This book wondrously simplifies the problem of explaining what has happened in the past and anticipating what will happen in the future. The trick, according to Stanislav Andreski, is to understand the psychological forces governing history. When confronted with momentous forces, people react at two levels. One consists of responses that are predictable, as long as the observer understands the appropriate psychological, psychoanalytical, and psychiatric mechanisms that govern human behavior. The other may include the unpredictable, which is merely to say that in mounting their entirely predictable responses to stimuli people may do entirely unpredictable things.

This line of reasoning allows the author to explain an array of historical events and movements whose causes have baffled students of the early modern period, and some other things, too. Puritanism; capitalism, hence also science and technology; the dying out of the nobility; wide acceptance of the idea of predestination; female resentment toward males; and witch hunts can all be accounted for, partially or completely, in this way. A momentous force, syphilis, provoked predictable psychological responses in human religious and sexual behavior—people became anxious about pleasures of the flesh—that had vast, unpredictable effects. Identifying causes like syphilis is simple. There must be chronological congruence. The putative human reactions must be plausible, in terms not of any specific school of psychology but instead within Andreski's sense of how humans are motivated and react. And the things explained in this way must otherwise lack satisfactory explanations.

It is of course doubtful whether any historical event could ever be explained in this way, much less whether puritanism, capitalism, witch hunts, and the rest should be. This is so not because psychological explanations lack merit, but because the specific form of explanation proposed in this book lacks merit. For Andreski it is enough to refer vaguely to such mechanisms as "the well known mental process of generalisation and extension" (p. 8) or "the psychological mechanism of generalisation and stereotyping" (p. 43), while accepting without reflection the idea that human psychological responses are constant over time.

The author provides a convincing demonstration of the silliness of his argument by adding an appendix on AIDS, a modern disease with some striking similarities to syphilis in the late fifteenth and early sixteenth centuries. Can psychological theory in this form anticipate human responses to AIDS? Andreski supposes first that AIDS will spread rapidly to larger proportions of the population, and that ultimately it will threaten human survival. As a protective reaction people will assign greater value to veracity and see deceit as a serious human failing. Unemployment will

diminish, health services will break down, and monogamous relationships will thrive, yet people will also use condoms. Authoritarian measures, such as segregating people infected with AIDS or even killing them, will be considered seriously and authoritarian societies will deal more effectively with the AIDS threat than will liberal societies. Rock music, which stimulates the passions, will give way to romantic and sentimental music. Gays, who Andreski suspects may control the British parliament, will lose their attempt to gain social acceptance. In addition to all these responses, which are called adaptive, people will react in irrational and unpredictable ways, like the witch hunts. "Even today," Andreski writes, "psychiatry remains the least effective branch of medicine and the most open to quackery" (p. 71).

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DAVID FREEDBERG. *The Power of Images: Studies in the History and Theory of Response*. Chicago: University of Chicago Press. 1989. Pp. xxv, 534. \$39.95.

With the heady expansion of cultural history in recent years, historians have moved across academic frontiers into anthropology, linguistics and literary criticism, cognitive psychology, and art history. The opening lines of this book read like an art historian's tribute, or capitulation, to this imperial project. "This is not a book about the history of art," David Freedberg writes. "It is about the relations between images and people in history" (p. xix). Freedberg dismisses connoisseurship, stylistic analysis, and iconography, the stand-bys of academic art history. His subject is the power of images to seem alive to "people," if not to "critics." He is interested in claims that images respond to the beholder's gaze and in the undeniable fact that they arouse deep-seated affective responses. In his book he has amassed and interrogated a vast collection of learning and lore on such responses to pictures, sculpture, prints, coins, votive offerings, fetishes, and charms from the East to (mostly) the West and from antiquity to the present. If the book were not such a large one, this could be cultural history *tout court*.

Yet, to adapt a powerful image, the book is also something of a Trojan Horse. While mounting a relentless assault on what he takes to be the formalistic and intellectualizing "repressions" and "denials" of art critics and art historians, Freedberg also wants to reclaim the power of images from historical relativism. His overarching conviction is that formalism and historicism have been partners in a genteel conspiracy against the enlivening presence that images evoke despite artificial formal distinctions between "high" art and "low" non-art and very real historical differences of time and place. Within what he insists is inductive proof based on any number of

examples, Freedberg holds in reserve a transhistorical, cross-cultural "experience" of privileged communication and heightened sensibility traditionally consigned to the category of art.

This tortuous strategy has paradoxical results. The attempt to universalize "our" experience of images as lifelike tilts the choice of examples to representations of the human figure. The alleged naturalness of this experience tends to remove it unnaturally from considerations of contingency and context. For Freedberg, images have power but hardly any politics because he is almost exclusively concerned with religious and erotic effects as if these were somehow apolitical. His emphasis on the expression of adoration, contempt, or fear leads to the repression of evidence for laughter, indifference, or disbelief. While stressing the cognitive value and communicative immediacy of images, Freedberg has little to say about their content, and for all the talk about responses, we are not told how images are actually made available for response in the processes of production, circulation, and exchange.

Not the least of its paradoxes is the fact that this deliberately iconoclastic book is so idolatrous. The glut of images in the modern (or postmodern) world may demonstrate our susceptibility, but it has also sharpened our critical instincts in the face of the image's power. Freedberg calls for a return to a lost state of innocence. One senses in this utopian proposition the spectacular bereavement of a discipline seeking the justification from history it has forfeited in art.

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DAVID KUNZLE. *The History of the Comic Strip: The Nineteenth Century*. Berkeley and Los Angeles: University of California Press. 1990. Pp. xx, 391. \$85.00.

For more than thirty years scholars have studied various manifestations of popular culture, from village ritual to television soap opera. Historians have learned that, like sports and sex, creative expressions in art, music, and literature at once reflect, distort, and resist larger historical forces, including that of industrial capitalism. Moreover, this work has been substantially seconded by another interdisciplinary interest in art, literature, and publishing. Social and intellectual historians continue their forays into domains once the preserves of art history, literary theory, and bibliography, whose specialists have returned the compliment. Art historian David Kunzle's latest book is a good example of these intersecting concerns. What Arnold Hauser has accomplished in his social history of art, Kunzle seeks to develop in his own history of the comic strip.

This volume is actually Kunzle's second on the topic, the first having appeared in 1973 (*The Early*

Comic Strip: Narrative Strips and Picture Stories in the European Broadsheet from c. 1450 to 1825). The present study traces the comic strip's development in its nineteenth-century context. For Kunzle the nation-state and the Industrial Revolution created the powerlessness, both real and perceived, that the comic strip's audience experienced in the face of political and economic change. In lieu of the didacticism typical of preindustrial broadsides and caricatures, nineteenth-century artists emphasized the entertainment value appropriate to their clientele, the petty bourgeoisie, "the bemused victims of incomprehensible fate and social forces" (p. 1). Rodolphe Töpffer, Cham, Léonce Petit, Wilhelm Busch, Marie Duval—to name only the most important—and their various imitators in England, France, Germany, and elsewhere in Europe defined a new art; and they did so in dozens of newspapers and special periodicals before other artists in the United States came to dominate the form in the twentieth century. But Kunzle is also attentive to the biographical and technical features of this work. With the circulation of Töpffer's *Cryptogame* in 1827, comic strip art developed a tradition of its own; creative individuals explored the artistic possibilities of an ephemeral form that captured well the discontinuity and fragmentation of modern social relations. Contemporary with the railroad, comic strip art shared "its fast, irregular pace, its frequent stops and starts (publication by installment), and its physical freneticism—which often escalated into bizarre and terrible accidents and extreme violence between people" (p. 377). Here form, content, and context were perfectly matched.

As thorough as it is well researched, Kunzle's work is encyclopedic. Its copious illustrations, often rare instances of the genre, accompany a text sensitive to artists and materials as well as to context and tradition. Kunzle makes no apologies for his Marxist approach that emphasizes economic and class relations. The weaknesses of this analysis are equally apparent. Kunzle fails to take seriously the other audience for this art that resists such ideological formulation: children. Moreover, Kunzle's ventures into many different cultures and national histories result in some annoying errors. For example, Frédéric Moreau, not Julien Sorel, is the principal character in Gustave Flaubert's *L'Éducation sentimentale* (p. 90); the French word *poire* (pear) connoted not a prick, but a dimwit, in the nineteenth century (p. 296); and the alliance between France and Russia arose not in the 1880s but in the 1890s (p. 178). Not only does Kunzle rely too much on textbooks but he also overlooks important monographs, such as L. P. Curtis's work on British caricature of the Irish (*Apes, Angels, and Irishmen* [1970]) and Peter Paret's essays in *Art as History* (1988). The book clearly deserved better copy editing than it received; there are too many typographical errors and awkward sentences. But these are minor criticisms of a major contribution to a

new interdisciplinary field. Like the volume that preceded it, this work merits the attention of social and intellectual historians alike.

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STANLEY BUDER. *Visionaries and Planners: The Garden City Movement and the Modern Community*. New York: Oxford University Press. 1990. Pp. xii, 260. \$32.50.

The elements of the modern metropolitan agglomeration were forged in the grim squalor and ecocatastrophe of the Industrial Revolution. And the solutions to the countless problems caused by urban manufacturing ranged from the clever packaging of soaps (Ivory Snow) to the radical restructuring of society itself (the *Communist Manifesto*). Removed from the minutiae of everyday palliatives, but falling somewhat short of the revolutionary measures suggested by Marx and Engels, came a tradition of utopian and communitarian thought that offered comprehensive remedies for the social and environmental degradation characteristic of the nineteenth-century city.

It is this Garden City movement and its struggles during the last century that are masterfully synthesized by Stanley Buder. At once a biography of British planning luminary Ebenezer Howard, a study of the consequences of laissez-faire markets for urban land, an examination of utopian and communitarian thought, and a skillful evaluation of the pragmatic winnowing of public policy from this complex set of antecedents, this work will interest those with concerns for city planning, housing, and for the mobilization of resources for public intervention in the urban environment.

Howard, the originator of the Garden City concept and its successor, the British New Towns movement, began his career as a stenographer and inventor, with time out for an unsuccessful Nebraska farming venture. But he was also one of the chief popularizers of Edward Bellamy's *Looking Backward* (1888). Although influenced by utopian thought, Howard's pragmatism reflected his interest in the rebuilding of Chicago following the fire of 1871 as well in Chicago's World's Fair of 1893. As a result, his work was directed at solving the vexing problems associated with urban areas rather than at grandiose communitarian schemes isolated from civilization.

Thus, Letchworth, the first Garden City, emerged from a web of basic concerns such as the cost of land and infrastructure, rail access to London, and the expertise needed to design and plan a town. The construction of Letchworth in 1907, with the aid of the brilliant planning team of Barry Parker and Raymond Unwin, proved to be a convincing statement of the importance of the Garden City concept that is still of relevance: to combine workplace and residence in a low-density planned environment lib-

erated from the excesses of land speculation and urban sprawl.

Although the role of government in planning and land use evolved somewhat later in the United States, Buder emphasizes Howard's impact on the creation of the British Town Planning and Housing Act of 1909. His focus then expands to the international impacts and critiques of Howard's achievements. Although other European societies were at best ambivalent about the low-density character of British Garden Cities, the concept found widespread acceptance in the United States where conditions were far more conducive to notions of environmental improvement in the absence of parallel societal transformations. Not surprisingly, American initiatives proceeded so as not to antagonize the business community. As a result, the American planning impulse springs from Progressive real estate developers seeking a secure and controlled environment for their ventures.

The book concludes with the impact of the Garden City movement in light of the Great Depression, Britain's postwar New Towns policy and efforts to control growth in its burgeoning southeast, and the New Communities Program that collapsed during the Ford administration. Such a contemporary failure underscores Howard's achievement as a monument to both his vision and his pragmatism.

This book will be indispensable for urban historians and for those concerned with the intellectual and practical impacts of the industrial city. The mixture of utopian thought, radical communitarianism, experimental colonies, environmental reform, and exigencies of housing and land economics will profitably occupy the reader. For those with general interests, the detail and illumination of the interconnections between these diverse forces will be a rewarding discovery. This book is valuable not only for its historical insights but for the contemporary relevance of ideas now more than a century old.

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HELEN ANGELOMATIS-TSOUGARAKIS. *The Eve of the Greek Revival: British Travellers' Perceptions of Early Nineteenth-Century Greece*. New York: Routledge. 1990. Pp. xvii, 289.

At the beginning of the nineteenth century, a considerable number of travelers from Great Britain made Greece and the Ottoman empire the goal of their journeys. Most were young aristocrats whose schooling in classical studies at Cambridge or Oxford encouraged them to seek out the places they had read about. They were also collectors who hoped to purchase antiquities to bring back to England or Scotland. From the beginning, some had in mind the intention of writing a book about their experiences.

Helen Angelomatis-Tsougarakis examines thirty-five books written by these travelers who went to

Greece during the years between 1800 and 1821. Her purpose is to find what information can be gleaned from their accounts concerning the land and people of Greece during that generation. From her research she has discovered a certain uniformity. Nearly all were avid diary keepers who wrote down their impressions daily, often on the spot. They usually traveled to the same places and stayed with the same people. Universally they found that the modern Greeks could not stand a comparison with their illustrious ancestors. Of all of the travelers, William Leake showed a certain sympathy toward the Greeks, and the author finds his books the most judicious.

The travelers wrote that nineteenth-century Greeks, especially in rural areas, were poor and barely able to survive. The loans they had to take out and the heavy taxes they were forced to pay were tremendous burdens. Over all of Greece the ominous figure of Ali Pasha of Ioannina loomed. Ali, an Albanian chieftain who was so secure that he could ignore the sultan's government, oppressed the people, squeezing out of them every bit of taxes possible.

Besides Ali Pasha there were other plagues. The travelers listed them as ignorance, the large Greek landowners who were able to profit from the Ottoman system, and, finally, Orthodox churchmen who were considered to be purveyors of superstition rather than religion. The author believes that these observations originated from the fact that the British, already opinionated, received their information from prejudiced sources, such as foreign consuls and merchants living in Greece. Apparently the travelers had little direct contact with Greek peasants, although they were often the major focus of the travelers' attention.

The small amount of revolutionary spirit found among the population is striking. Some Greeks who had spent time abroad spoke of deliverance by a Western power, but in the population as a whole the travelers discovered little preparation for an armed insurrection against the Turks.

Angelomatis-Tsougarakis has composed a thorough work with closely packed information that offers the reader a kind of encyclopedia of prerevolutionary Greece, albeit from a foreign point of view. At times, she introduces a guarded opinion on the credibility of the travelers' observations, but for the most part she allows them to speak for themselves.

Two elements, in my opinion, would have strengthened her presentation: the introduction of more detailed maps of the places and regions mentioned and a comprehensive list of the travelers, informing the reader more about their backgrounds, their ages when they went to Greece, and how many copies of their books were printed. Knowing more about the travelers would put their reports into better perspective.

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JACQUES PORTES. *Une Fascination réticente: Les Etats-Unis dans l'opinion française*. Foreword by CLAUDE FOHLEN. Nancy: Presses Universitaires de Nancy. 1990. Pp. 458. 230 fr.

In 1947, Jean-Paul Sartre proposed that no visitor to America could ever make sense of the contradictions attending the nation's confidence in the myths of liberty, progress, happiness, and equality. The French tourists, writers, and citizens who journeyed to the United States between 1870 and 1914 were largely free of Sartre's unqualified ironic response to American society. In the same fashion Jacques Portes judiciously and imaginatively presents the pleasures, disappointments, criticisms, and feelings of delight and admiration shared by French visitors to the United States. His book begins by first presenting the simple, least-sophisticated responses of the French traveling in America. These commonplace reactions are then followed by attention to the more sophisticated interests and judgments of scholars, doctors, women, journalists, and political commentators.

Not surprisingly, American food was universally found to be abominable. Having accepted this trial, the French tourists then bravely and with stern purpose crossed the continent, showing great interest in St. Louis, Baltimore, New York City, Washington D.C., Chicago, and San Francisco. With notebooks in hand, or at least the intention to remember what was to be retold at home, the French and particularly the learned among their company paid careful attention to the social issues confronting the American nation. They sought, for example, to understand the place of "les nègres" in America, but had little knowledge of the actual situation of America's African-American citizens. The French were more serious and more assured in their conclusions concerning America's industrial growth, commercial zeal, flamboyant press, passion for sport, and expectation that its people would somehow survive a life characterized as "truquée et machinée" (p. 127).

As citizens of the Third Republic, which was not without its problems and scandals, the French observing the American scene quickly recognized what they considered to be the mediocrity of American politics. At the same time they were bedazzled by the person and reputation of Theodore Roosevelt and they held in great respect the works of Woodrow Wilson on American political institutions. The French were impressed by America's educational systems, by the free access to its primary schooling, by the secular character of the schools, and particularly applauded the scope, accessibility, and quality of America's universities. In this same appreciative mode, the French journeying in America deeply respected American women for their spirit, independence, and taste, a praise similarly expressed at this time by Henry James in his essays and novels.

The French, in attempting to observe America's religious practices, only superficially made contact

with the Protestant communities. Catholicism in America was thought to be fortunate to have escaped the constraints imposed on its people by the church in Europe. In somewhat the same way, labor and the unions were seen as unlikely to experience the cataclysmic class struggles predicted for Europe.

In the final section of his splendid history Portes raises the question the French felt most obliged to ask: "Y a-t-il une culture aux Etats-Unis?" Whatever the answer, in this century or the nineteenth century, all future discussions of the French ways of seeing America must take as their guide Portes's model work.

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Madison*

ROBERT C. HILDERBRAND. *Dumbarton Oaks: The Origins of the United Nations and the Search for Postwar Security*. Chapel Hill: University of North Carolina Press. 1990. Pp. x, 320. \$39.95.

And then there was one. With the publication of this solid, detailed narrative on the Dumbarton Oaks Conference, only Casablanca among the major World War II conferences awaits its own historian. If nothing else, such comprehensive monographic coverage of virtually every major wartime event related to great power relations should allow historians to concentrate anew on broader issues or less-studied subjects. A full opening of Soviet Foreign Ministry archives (instead of the current custom of divulging snippets of out-of-context "revelations," often at conferences and dinners) could stimulate a new wave of monographs, but more important will be what those archives reveal about the broad development of Soviet perspectives of the wartime alliance and postwar planning.

Robert C. Hilderbrand does more than merely fill a small gap. His intensive research in British and U.S. archives and literature provides a comprehensive Anglo-American view of the day-by-day conduct of the conference. It is a case study in great power negotiations over the restructuring of the international order, perhaps more relevant to today's world than cold war curiosities such as the Panmunjom talks.

Nevertheless, the book must be read within the broad context of policy and diplomacy during the war. The Big Three and even Chiang Kai-shek loom behind the scenes, while the leads at the conference were all actors used to playing supporting roles. Edward Stettinius, then undersecretary of state, lives up to his popular image of a bumbling, well-intentioned devotee of what he believed was Wilsonianism—although it is difficult to take seriously a man who would ask the dour Alexander Cadogan, the British delegate, to explain the Dumbarton Oaks Conference to the Radio City Music Hall Rockettes.

The assignment of Andrei Gromyko, then a young diplomat, as the Soviet representative brought fears that the Russians were downgrading the conference. Roosevelt insisted that China be included, which annoyed Churchill, brought opposition from Stalin, and did nothing to enhance China's status.

In fact, if any conference should have commanded the presence of the world leaders, it was this one, for it dealt with the crucial issue of postwar security. Summit diplomacy has been much maligned for the public pressure it creates to deliver great breakthroughs. Dumbarton Oaks may be a negative example of why summit diplomacy can be an effective route. Perhaps arguments over the veto, the "X" issue (membership in the United Nations for additional Soviet republics), or regionalism could have been resolved there, without letting them fester and deepen tensions. Instead, solutions had to await the Big Three meeting at Yalta. Moreover, Stettinius did not comprehend Roosevelt's conception of the best, practical postwar order. The fault for that lay at the president's door, but so did much of the solution.

Despite a series of fits and starts, Dumbarton Oaks helped create a United Nations organization based fundamentally on the great power structure rather than one built on international law and due process—and Hilderbrand describes well how that path was taken. The organization was deeply dedicated to the preservation of the status quo, so long as the Big Three (plus one or two) could agree on what that meant. It was the path that Churchill, Roosevelt, and Stalin preferred, with as few fellow travelers as possible. The nationalism, in the form of interests and security, that surfaced at Dumbarton Oaks prevailed by war's end, as it has for two hundred years. Hilderbrand puts it eloquently: "The vocabulary of peace, so often employed during the war, had been replaced by the language of national security—and it was almost impossible to make a coherent translation of one into the other" (p. 256).

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DENNIS MERRILL. *Bread and the Ballot: The United States and India's Economic Development, 1947–1963*. Chapel Hill: University of North Carolina Press. 1990. Pp. xiv, 282. \$39.95.

Dennis Merrill's work nicely complements economist George Rosen's book, *Western Economists and Eastern Societies* (1985). Both focus on American aid programs that were intended to promote economic development in India. Rosen examined the activities of academic experts and philanthropic foundations. Merrill, using the sources and framework of diplomatic history, examines government-to-government relations. Their conclusions point in the same direction: American efforts to aid India stemmed from a

global strategy in fighting communism during the cold war, and for those who applied that strategy India never ranked high on the list of priorities.

Without neglecting the Indian side of the story, Merrill concentrates on the politics and perspectives of policy makers in Washington, D.C. He finds that the administrations of Harry Truman, Dwight Eisenhower, and John Kennedy all harbored hegemonic ambitions and suffered from cold war myopia regarding the regional complexities and realities within south Asia. All three administrations had to confront a Congress that was indifferent, skeptical, or firmly opposed to economic aid for nonaligned India. To sell their aid programs they used hard-headed arguments that promised practical benefits for the United States.

Readers looking for economic motives underlying U.S. policies will be disappointed by Merrill's analysis. To be sure, he shows that policy makers occasionally talked about fostering capitalism and trade beneficial for the United States, and they often regretted India's preference for public sector investments. Some also tried to use the food aid program to reduce American agricultural surpluses, once offering to include six million tons of tobacco to India when it faced a famine. But no one dreamed about an "India market" vital to the United States. Merrill leaves no doubt that, for the United States, economic aid served primarily as a cold war weapon to keep India and other non-Western countries from turning to communism and alliance with the Soviet Union or China.

Although all three administrations shared common policies, Eisenhower's comes off as most successful in dealing with India. Overcoming Republican fiscal conservatism, he and Secretary of State John Foster Dulles decided that the spread of the cold war into the Third World required that the United States substantially increase economic aid for India, even though it refused to abandon nonalignment. Such policy makers, however, put too much faith in the can-do spirit of American development experts and underestimated the obstacles to progress within India. At one point, for example, Eisenhower expressed hope that India and Pakistan would merge. President Kennedy repackaged the policy with rhetoric about making the 1960s the "development decade," but by 1962 his administration's exaggerated hope for making India the bulwark for U.S. policy in the non-Western world began to fade.

Merrill has written a good book based on solid research. He might have considered the American role in United Nations economic development efforts, but the story would not have changed. His book confirms that India never enjoyed a significant constituency in the United States. Advocates of economic aid fit their case in a cold war framework with simplified or distorted perceptions and expectations. The east-west mentality of U.S. policy makers persisted into an era when the non-Western countries

were beginning to demand changes in north-south relations.

ALAN R. RAUCHER
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SCOTT L. BILLS. *Empire and Cold War: The Roots of U.S.-Third World Antagonism, 1945-47*. New York: St. Martin's. 1990. Pp. xii, 280. \$35.00.

Scott L. Bills examines a crucial element in the often stormy relationship between the United States and the Third World. Focusing on the U.S. response to independence movements in North Africa, the Near East, the Middle East, and Southeast Asia in the turbulent period from 1945 to 1947, Bills shows how and why the admiration felt by nationalists for the United States turned to disillusionment and distrust. The U.S. government, he argues, had "no interest" in underwriting "global revolution" (p. xi) and, in short order, threw in its lot with the European colonial powers. Although some U.S. officials, including President Franklin Roosevelt, did assume that imperial rule was corrupt and oppressive, they did not create "a consistent, viable US policy which can be called 'anticolonialism'" (p. xi).

Instead, U.S. policy remained "Eurocentric" (p. 21). In May 1945 the U.S. secretary of state assured the French foreign minister that the U.S. government was not "questioning, even by implication, French sovereignty over Indo-China" (p. 81). Establishing "any Moslem state" in Algeria, the U.S. consul general reported in 1946, "is presently unthinkable" (p. 25). To be sure, the State Department occasionally promoted what Bills calls "liberal colonialism" (p. 192)—initiating reforms and training the locals for self-government. But this had little appeal to either the restless native masses or to the intransigent European powers, who, as in Indonesia and Indochina, masqueraded behind reform rhetoric while deploying their troops to massacre the native peoples in a futile attempt to reimpose colonial rule. America's "Europe-first strategy," Bills observes, became particularly rigid with the intensification of the Soviet-American confrontation. In this "ideological context which identified colonies via the light reflected from the metropolises," the "simplistic cold war theses always prevailed" (p. 199), much to the detriment of national independence.

This is a richly researched, well-argued book. Bills draws heavily on U.S. government records and also uses British government records, the papers of American officials, oral history interviews, and a broad array of published works. At times the book may strike readers as somewhat diffuse, for the writing is not always tightly structured and the work surveys events and the attitudes of observers in dozens of nations. But this is compensated for by illuminating detail and imagery, as well as by a nicely

focused conclusion. Overall, the book provides a powerful and convincing account.

Although Bills analyzes the role of the cold war in strengthening America's alignment with the European colonial powers, he is less specific about the reasons for its earlier Eurocentrism, especially during the Soviet-American honeymoon period of World War II. To what degree, one wonders, was the American preference for Europe over the Third World a product of traditional military, economic, cultural, and racial values and concerns? The author has not provided an answer to this question, but this thought-provoking study should encourage other historians to address it.

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GLEN ST. J. BARCLAY. *A Very Small Insurance Policy: The Politics of Australian Involvement in Vietnam, 1954-1967*. New York: University of Queensland Press; distributed by I.S.B.S., Portland, Oreg. 1988. Pp. 199. \$26.95.

In 1962, a small Australian army contingent was sent to Vietnam, but when U.S. pressure mounted the Conservative government of Robert Menzies announced in 1965 the commitment of a battalion to South Vietnam, stressing the argument that Australia had to pay its way in its alliances. The alliance involved was presumably SEATO, and Canberra was understood to be providing military assistance because Saigon had asked for it, as it was entitled to do under the terms of the SEATO protocols. The truth is that Canberra was more probably paying the premium on the U.S.-Australia-New Zealand Tripartite Security Treaty (popularly known as ANZUS), the cornerstone of its foreign policy since 1952.

A total of fifty thousand Australians served in the conflict until 1972 when the Labor government of Gough Whitlam recalled the remaining personnel. And, even setting aside the acrimonious debate surrounding the reintroduction of military conscription, the impact of Australian involvement in Indochina was heavy: 400 dead, 2,500 wounded, and a total expenditure of \$A162 million.

Most Australian literature on this subject portrays Canberra as the willing junior partner of the United States in Southeast Asia, ultimately dependent on Washington for its national security and making Prime Minister Harold Holt's famous toast "All the Way with LBJ" at least intelligible. Glen St. J. Barclay's slight book (169 pages of text divided into seven chapters) falls well within this category, with one notable exception. Barclay argues that "there was no question of Australia obsequiously following the guidance of great and powerful friends" (p. 167). In this sense Canberra pursued the traditional Australian apprehension of a threat from Asia, that is,

concern about the hegemonic southward ambitions of the People's Republic of China, in earlier times derisively referred to as the "Yellow Peril."

Conceding that there was no way Australia could have compelled the United States to intervene militarily in Vietnam, Barclay makes the novel case that what Australia could do was provide the element that successive administrations in Washington had described as the absolutely essential precondition for U.S. involvement: regional support by a credible democratic government with interests of its own and with the capacity to act independently in support of them. "It was not a matter of an Australian tail wagging the American dog. It was a matter of providing [Lyndon] Johnson with an assurance of support that had not been available to Eisenhower, so that he would not feel required to draw back from the brink" (p. 168). It is an interesting interpretation but one that will not hold up to close scrutiny, as will be obvious to any scholar with a basic understanding of U.S. source materials. It is not the first time, however, that the pen has wagged the writer Down Under.

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Australia

ANCIENT

ANDREW ERSKINE. *The Hellenistic Stoa: Political Thought and Action*. Ithaca: Cornell University Press. 1990. Pp. xi, 233. \$37.50.

Like the cycles of the year, the notion that ideas are efficacious in politics constantly returns because it is a comforting illusion in a world of self-interest and endemic folly. In fact, as the American humorist Don Marquis quipped, "when a politician does get an idea, he usually gets it all wrong." Moreover, the willing prostitution of intellectuals to those in power is abundantly demonstrated by examples ranging from Plato and Philistus at the court of Dionysius to Giovanni Gentile, Martin Heidegger, and a host of our contemporaries. Although most Stoics were dour defenders of the status quo, some modern idealists concocted an image of Stoicism as a proto-Christian-Socialism shining down the ages. After World War II, this mirage was dispelled by advocates of realism in Greek and Roman history.

Andrew Erskine has resurrected the corpse of Stoic socialism to reenact the drama of philosophers in action as champions of democracy and foes of slavery. His book is learned and provocative with valuable comments on the connection between philosophic notions of psychology and political theory. He concedes that ancient sources are shaky and that ideas must be viewed in historical context, but his intellectualist bias leads him to overrate the value of consistency (Ralph Waldo Emerson's "hobgoblin of little minds") in reconstructing lost texts. Like history, theory is rarely tidy. Erskine is also selective in dis-

missing social contexts (for example, unstoic behavior by Stoics or Roman aristocratic *mentalité*) that impede his thesis. Whether re-creating ideas or assessing historical "facts," he engages in close readings of cobbled texts and blithely moves from "possibly" to "probably" to "fact" in a couple of pages. The history of the Hellenistic era is woven of gossamer threads that simply will not bear the weight of so much certainty. The convoluted lives of modern intellectuals should warn against inferences about what Greek sages "must have" thought.

According to Erskine, Zeno's *Politeia* was a work of his mature years, not a youthful indiscretion influenced by Cynics. In this utopia of wise men only, property and women were shared and there were no kings or slaves. Although most humans are "bad," some could attain Stoic enlightenment and might apply Zeno's speculative notions to real life. Because the Stoic utopia was "democratic" and Macedon was not, the early Stoa "must have been" for Athenian democracy and against Macedonian monarchy. Erskine rejects tales of Zeno's contacts with Antigonus Gonatas and dismisses Perseus, whom Zeno sent to grace the Macedonian court, as not a "true Stoic." In the Chremonidean War against Macedon, he senses "Stoic influence" where others see Ptolemaic intrigue and Athenian habit. (When Antigonus occupied Athens in 261 B.C., the king did not molest Stoics, but he did execute a real anti-Macedonian democrat, the historian Philochorus.) The linchpin of Stoic socialism was Sphaerus, who wrote propaganda for the Spartan revolution and may have advised Cleomenes III. (Sphaerus relished royal patrons and wound up at the court of Ptolemy IV.) Erskine insists that the program of Agis IV has been confused with that of Cleomenes, that is, Stoic communism, and that much of what passes for "Lycurgan equality" in Spartan tradition comes from Sphaerus's tracts. Because Cleomenes' agents played on social-economic unrest in Achaean cities, the Spartan revolution "must have been" intended for export, although the king did not deliver on his promises to abolish debts or redistribute property in Argos, and he abandoned the "cause" after the defeat at Sellasia. Although many scholars see Cleomenes as an opportunist, Erskine is sure that his motives were Stoic. Because of Sphaerus's embarrassing involvement with Sparta, the later Stoa backed away from "Zeno's" radicalism and became apologists for private property and Roman domination. One exception to the Stoic retreat was Blossius, the confidant of Tiberius Gracchus, who turned the tribune into a Stoic "democrat" and brought him to ruin. (Tiberius was the headstrong grandson of Scipio Africanus and his land program had powerful consular and family backing.) Erskine believes that "the manpower shortage is a modern construct" (p. 170) and he rejects Appian's view that Tiberius wanted to turn proletarians into landholders to draft them as soldiers—whom he would lead to glory. (If Blossius was a Stoic radical, however, why did he

bypass the rebel slave state in Sicily to serve the Pergamene pretender, Aristonicus, whose anti-Roman rising was more Jacobite than Jacobin?) Because any appraisal of Cleomenes or Tiberius Gracchus must rely on Plutarch's moralistic reworking of novelistic sources, the appearance of "Stoic" vocabulary in made-up speeches is hardly conclusive evidence of intellectual influence on the originals. When he reconstructs the probable views of formal philosophers, Erskine is often persuasive, but his versions of historical context and human behavior are not likely to replace the realism that has prevailed in academic circles since prosopography ousted the "conflict of ideas."

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PETER GREEN. *Alexander to Actium: The Historical Evolution of the Hellenistic Age*. (Hellenistic Culture and Society, number 1.) Berkeley and Los Angeles: University of California Press. 1990. Pp. xxiii, 970. \$65.00.

This book is one of the most fascinating works I have ever read. Where else in learned treatises are Bertie Wooster and Jeeves appositely cited (p. 73), or is reference made to Mick Jagger of the Rolling Stones (p. 879)? Peter Green has written widely on Greek history, and here he has created a masterpiece. The work is intended for general readers, but on page two they may blink at meeting an unidentified character, Peucestas, or at the information that the great Cleopatra was the "last Lagid monarch." Foreign words and phrases also appear frequently. The footnotes, which display the author's command of the sources and modern literature, are another matter; quotations in Greek, Latin, and European languages are given without stint as deemed useful.

The basis of the survey is chronological and political. Green does his best with the often inadequate sources and is deft in swift characterization. He describes Antigonas Gonatas, for example, as "that most unlikely of Macedonian monarchs . . . a ruler whose concept of his duties (similar in this to that other Stoic, Marcus Aurelius) verged on the masochistic" (pp. 140–41). Yet these chapters cannot carry one far beyond Edouard Will's magisterial *Histoire politique du monde hellénistique* (1966). On the major problems Green is judicious and thoughtful. The concept that the Hellenistic world witnessed a deliberate merger of Greek and Near Eastern cultures gets its proper short shrift (pp. 312 and following), although his pages are devoted almost entirely to the Hellenic elite. Nor does he make the Roman conquerors and looters any worse than they really were; after Pompey's reorganization of the East, the Romans "simply made the system work" for several centuries (p. 661). Not everyone, however, as Green notes in his

preface, would agree with his depreciation of the autonomy of the Hellenistic *poleis*.

At many points Green turns aside—one almost feels with relief—to magnificent panoramas of the cultural developments of the age, presented in delightful, reasoned assessments. Without slighting other examples, one might cite his humorous but thoughtful appraisal of Menander, his well-illustrated accounts of the stages of Hellenistic art (in the first stage, oddly enough, he misdates the discovery of the Hermes of Praxiteles at Olympia and transfers it to Samos), even his careful analysis of Hellenistic mathematics. (He admits in his preface that "the Greek treatment of quadratic equations or conic sections is not, in detail, my business.") Such discussions in lesser hands might be potted essays, but Green, while freely admitting indebtedness to others, steers his course skillfully.

Detailed chronological and genealogical tables, notes (usually references but at times assessments of modern views), bibliography, and index take up almost one-third of the book. "Big book, bad book," warned Callimachus, but in truth one would not wish to remove much from these pages except perhaps the overly abundant cross references. Students, however, would be advised to sip, not gulp.

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G. HOFFMANN. *Le Châtiment des amants dans la Grèce classique*. (Antiques.) Paris: Boccard. 1990. Pp. 165. 150 fr.

The classical city-states of Greece did not punish adulterers with equal severity. In Athens, according to a law attributed to Solon, it was permissible to kill a man caught in the act of adultery. In Gortyn the seduction of another man's wife was treated rather like a theft of his property, whereas in Sparta it was denied that crimes of this sort ever took place. Elsewhere an adulterer might suffer the poetically apposite indignity of being sodomized with a vegetable. G. Hoffmann's aim is to analyze the ideology behind these different forms of punishment and, in the case of Athens at least, to offer what he describes as a "political perspective" on the treatment of adulterers. In addition, he investigates the image of the adulterer in both literature and art, providing a detailed portrait of Alcibiades, the most celebrated of all male Greek adulterers.

This subject, whether addressed within the circle of one's own culture or in other cultures, is one that evokes a *frisson* of prurient fascination, and it is treated by the author with a certain esprit. It is regrettable that there is so little evidence to go on, a fact revealing more about attitudes toward adultery than the author acknowledges. The only adulterers whose reputation and behavior we can examine in

some detail turn out to be Paris and Alcibiades, hardly a representative sample. (It would have been useful to have some discussion of Homer's portrayal of the consummately poised Helen in *Odyssey* 4, and of Ares's and Aphrodite's ill-fated amour in *Odyssey* 8.) Similarly, the only extensive description of a real-life act of adultery in an Athenian context is supplied by Lysias's oration *On the Murder of Erasthenes*. Hoffmann makes much of this justly celebrated incident, which took place shortly after the restoration of Athenian democracy, arguing, somewhat exaggeratedly in my view, that the description of Euphiletos's murder of his wife's lover, caught *flagrante delicto*, "constituted a plea that was capable of exploiting the consensus of the age and the condemnation of partisan egoism" (p. 21). Commendable though it is to attempt to situate this incident *sociologiquement*, it is not altogether convincing to assume that an Athenian jury would have felt much differently toward the crime of adultery under an oligarchical, rather than under a newly restored democratic, regime. In conclusion, this is an entertaining monograph fueled by an enthusiasm for its subject, which makes all it can, or perhaps rather too much, of the somewhat meager evidence.

ROBERT GARLAND
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NATHAN ROSENSTEIN. *Imperatores Victi: Military Defeat and Aristocratic Competition in the Middle and Late Republic*. Berkeley and Los Angeles: University of California Press. 1990. Pp. xii, 224. \$28.00.

In this slim volume, the view is clearly set forth that fiercely competitive aristocrats of the Roman Republic routinely could not or did not employ against candidates for political office the issue that opponents had suffered military defeats. Nathan Rosenstein sees this occurrence as partly the result of Rome's reliance on and faith in the *pax decorum*: harmony between Rome and its gods assured victory, and its absence helped to account for defeat. Another factor contributing to Rome's victories was the valor of its soldiers. Here, too, if discipline or courage failed, commanders were not always held responsible. Finally, the aristocratic code guaranteed that if commanders personally acted honorably, defeat was not a political liability. Rosenstein discusses the careers of defeated commanders and shows that they were no less likely to gain subsequent offices than victorious leaders and no more likely to be hauled into court. Much of the discussion is on the mark, but the historical context in which Rosenstein places the origin of this "system"—*victi* were stigmatized before 390 B.C. when there was "an abrupt shift in outlook" (p. 158)—and the interpretation of subsequent changes in it, leave much to be desired.

There is only a modest evaluation of the sources and little regard for the difficulties involved in work-

ing with descriptions of events centuries removed from the incidents reported. Like many others, Rosenstein postulates second-century changes without fully appreciating the extent to which change is the product of moralizing about Rome's imperfectly documented and poorly understood past by second-century authors who were the first to chronicle events. The author's faith in the "commonplace" that Roman magistrates were not elected on the basis of their military prowess is unwarranted and does not follow from the fact that *victi* were subsequently elected to magistracies. All magistrates with *imperium* commanded (from *imperare*, "to command"), and, until the second century, commanding was tolerably all that the military assembly, *comitia centuriata*, elected them to do. Also, an examination of all commanders indicted, even victorious ones, would have revealed occasional antagonism between civilians or soldiers and commanders. I found no discussion of Livy 8.33.17–18, which is clearly germane to the thesis, but that *victi* were not uniformly stigmatized does reveal less about the military competence of magistrates than it does about the nature of a political system firmly in the control of privileged aristocrats.

Rosenstein endorses the idea of an aristocratic consensus or "gentlemen's agreement" that played down military prowess and argues that the system was based on the "myth of universal aristocratic competence" (p. 172). Presumably, all aristocrats were equal, and competence did not focus on military success because that would allow a few individuals to monopolize offices, an ever-present threat to the aristocracy. Rosenstein sees the decline in the iteration of offices as the consequence of a conscious aristocratic plan to broaden, however slightly, the pool of aristocrats. It seems more likely that upward mobility by *novi* was the result of support by patrons whose interests were served, and iteration clearly declined as a consequence of the rise in promagistracies. The author has revealed significant details about Roman political life; unfortunately, his explanation and interpretation of them are less certain.

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KEITH R. BRADLEY. *Slavery and Rebellion in the Roman World, 140 B.C.–70 B.C.* Bloomington: Indiana University Press or B. T. Batsford, London. 1989. Pp. xiii, 186. \$27.50.

This outstanding book is likely to become the standard reference work on Roman slave wars for years to come. It is also in many ways a highly useful general social history of Rome in the second and first centuries B.C. Not the least attraction of Keith R. Bradley's work is that it suggests new ways of thinking

about the subject of Roman slavery and slave rebellions by offering parallels with the maroon societies of fugitive slaves in the New World.

Each of the three core chapters of the book is presented in the context of some aspect of slavery or the social history of the period 140–70 B.C. Chapter 3 (The First Slave War in Sicily) begins with a general description of the living conditions of slaves in Roman Italy: their diet, housing, clothing, and the treatment they could expect to receive under varying conditions. The revolt of Spartacus (chapter 5) provides excellent vignettes of the conditions at Capua where the revolt broke out and of the social psychology of gladiators. Of special interest is the description of the way gladiators fit into the Roman tradition of self-help and how they were used to settle public as well as private grievances at Rome.

Chapter 6 (The Maintenance of Rebellion) sums up the revolts. Bradley finds similarities in all three. No revolutionary atmosphere sparked the revolts and none presented a threat to the institution of slavery itself. They were rebellions pure and simple, individual acts of desperation by first generation slaves in large populations of first-generation slaves. Escalation of the revolts was, in each instance, unforeseen, unplanned, even unwanted. In fact, it was precisely the problem of large numbers that brought down the revolts. As the numbers increased the leaders found themselves hard-pressed to feed, arm, and discipline their followers. Once a certain threshold of numbers was passed Rome always responded with devastating force. There was never sufficient time for slaves to achieve the semipermanence of maroon societies, nor did the geography of Italy or Sicily favor them as geographical factors did their counterparts in the New World. That the revolts succeeded to the extent they did was due to the extraordinary charismatic leaders who emerged, their use of magic, divination, prophecy, and miracles, as well as the symbols of legitimate authority borrowed from both Hellenistic and Roman sources.

There are a few points on which general agreement is unlikely. The discussion of slave reproduction is not altogether convincing, especially given the massive disruption of the slave wars and the slaughter of the slaves themselves. Contemporary examples might be cited. For instance, it has been estimated that as many African slaves were transported to the Middle East as to the Americas, yet it was only in the Americas that they reproduced themselves. Can we therefore be sure that the Roman slave population of Italy did indeed reproduce itself? Is it possible that the second and first centuries B.C. were exceptional and that the disappearance of first-generation slaves and a decline in the numbers of slaves was the reason why there were no slave revolts after 70 B.C., rather than that the slave owners had learned something from experience, as Bradley suggests? The issue is

unfortunately insoluble and in no way detracts from the merit of this excellent book.

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LLOYD A. THOMPSON. *Romans and Blacks*. (Oklahoma Series in Classical Culture, number 2.) Norman: University of Oklahoma Press. 1989. Pp. xii, 253. \$32.50.

This new study of a subject that has gained much attention in recent years was inspired by the author's reading of F. M. Snowden's pioneering work, *Blacks in Antiquity: Ethiopians in the Greco-Roman Experience* (1970). Lloyd A. Thompson has made numerous contributions to learned journals in the field of ancient history, notably his *Africa in Classical Antiquity* (1969), edited with John Ferguson. The subject of the present book requires familiarity with ancient art, archaeology, classical literature of all kinds, patristics—the list is long, yet Thompson has also managed to immerse himself in comparative material from modern history, anthropology, sociology, and psychology. The time span of the book, announced on the dust jacket, is “first century B.C. to the third century A.D.” Thompson in fact covers far more: note, for example, the fascinating case of the black monk Moses and the use made of the Vandal-era North African poet Luxorius.

The work is concise and readable, packed with information and ideas (enough to inspire much future work). After a short introduction come three main chapters: “Review of the Modern Literature,” “The *Aethiops* Type in Roman Perceptions,” and “The Evidence in Its Ideological Context.” Each chapter is subdivided: chapter 3, for example, has eleven headings.

The wealth of material Thompson provides is such that a reviewer can only discuss a little in detail. One may register a mild protest at his uncritical use of the *Historia Augusta* on the pretender “Firmus” (bogus) and Queen Zenobia (for example, p. 73). Even in its better sections the *Historia Augusta* is full of pitfalls. Thompson, like Snowden, quotes the story in the *v. Severi* of the “Ethiopian” soldier in the British army who upset Septimius Severus. But Thompson, unlike Snowden, uses the Loeb text, which starts with Peter's unnecessary emendation “post murum apud Luguvallum visum,” instead of the manuscript “post Maurum apud vallum missum.” That, as Snowden showed, makes good sense (note a *numerus Maurorum* stationed near the presumed point where Severus crossed the frontier, *vallum*). But the whole story may be bogus (although still worth discussing): Thompson himself cites a string of stories with similar “omens,” of which that in Plutarch, *Brutus* 48, is the best known. This point leads one to suggest that if the publishers decide on a second edition, they might usefully arrange for an index of sources to be added, which

would facilitate checking the vast range of references in Thompson's notes. Finally, a suggestion for a further line of enquiry: Roman color-names. I. Kajanto in *The Latin Cognomina* (1965) provided a useful basis: Rufus/a and derivatives were far and away the most popular, but Niger and Fuscus were much favored too.

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MEDIEVAL

ROSAMOND MCKITTERICK. *The Carolingians and the Written Word*. New York: Cambridge University Press. 1989. Pp. xvi, 290. Cloth \$54.50, paper \$17.95.

The word "literacy" is currently being used in a variety of contexts—computer literacy is but one example—that would have it indicate a variety of not very well defined intellectual competencies. It is worth noting, therefore, that for Rosamond McKitterick the word means the ability to read and write. Her important new book, covering Frankish Europe from the eighth to the tenth centuries, argues that Carolingian society "was a society to which the written word was central" (p. 273) for secular as well as for religious purposes.

McKitterick begins with a brief consideration of written and spoken language, noting that recent scholarship has retarded the date at which Latin was perceived as being a language different from spoken Romance languages until at least the ninth century, and that even in German-speaking regions Latin was used so extensively as a language of record that familiarity with it could be taken for granted among those who participated in public life. "Latin was, therefore, not the obstacle to literacy that it has hitherto appeared" (p. 22). Here and elsewhere in the book, McKitterick's discussion is confined to northern Europe, with greater attention given to Anglo-Saxon Britain than to Frankish Italy.

In the next two chapters, McKitterick develops this thesis by careful scrutiny of a variety of evidence. Chapter 2 enhances a discussion of the Carolingian government's use of written communication by a consideration of surviving codices of Frankish law, using the other contents of the codices as a guide to the intentions of the compilers. McKitterick finds that most of the codices were law books, that is, they contained other legal materials, rather than miscellanies, and she adds that many volumes show signs of wear and tear that indicate regular use. Chapter 3, devoted to the charters of St. Gall, is methodologically the most original in the book, drawing still more important conclusions from the physical form of surviving evidence. Some letter forms, for example, appear to derive from Roman cursive, suggesting a continuity of writing during the early Middle Ages, whereas the diversity of scripts that appear in the charters indicate that St. Gall possessed a monopoly on neither the teaching nor the use of writing.

The following two chapters focus specifically on books. Chapter 4 considers books—how they were produced, how much they cost, and who owned them. Chapter 5 treats monastic libraries, including not only the books owned but the categories used when monks prepared catalogs of their collections. Chapter 6 addresses the literacy of the lay nobility, the only group of laymen and laywomen for which evidence exists in any abundance. McKitterick does not venture quantitative estimates, notoriously uncertain in any case, but she shows that a variety of avenues existed through which lay people could acquire basic reading skills, that Carolingian court poetry seems to have included lay people among its audience, and that many surviving manuscripts are known to have had lay owners.

McKitterick's analysis has implications beyond the material that she specifically discusses. The existence of cursive apparently derived from antique forms poses the question of how much writing survived in Merovingian centuries, a problem to which she calls attention. Moreover, her contention that Carolingian society was meaningfully literate suggests that it is incorrect to see the cultural changes of the eleventh and twelfth centuries simply or primarily as a shift from orality to literacy.

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ELEMÉR MÁLYUSZ. *Kaiser Sigismund in Ungarn 1387–1437*. Translated by ANIKÓ SZMODITS. Budapest: Akadémiai Kiadó. 1990. Pp. 421; 36 plates. \$35.00.

The traditional assessment of Sigismund of Luxemburg's fifty-year rule in Hungary has been rather simplistic and, on the whole, negative. In the past several decades, however, a more complex, nuanced, and somewhat more positive picture has been emerging, based largely (although not exclusively) on the work of scholars in Hungary. No one was more important in this process than the late Elemér Mályusz, who was the great Nestor of Sigismund studies. His contributions began to appear in the late 1920s, and in the 1950s he edited the multivolume set of sources that provides the indispensable basis for the study of Sigismund's era in Hungary. Subsequent articles and books addressed specific topics, and, in 1984, the Hungarian version of this volume appeared. It represents the culmination of a lifetime of work and provides a comprehensive picture of the reign of Sigismund.

Mályusz's study is not so much a biography of Sigismund as an analysis of the political, economic, social, and cultural history of the ruler's time. He nevertheless pays particular attention to the details of the king's involvement in the developments of this period and attempts to assess the degree to which Sigismund was responsible for them. The book begins with two long chapters devoted to the political

history of Sigismund's reign. This part of the study is particularly good at tracing the careers of individuals and showing how Sigismund's "Order of the Dragon" (founded in 1408) constituted both a support for his own person and a limit on royal power in general. The section on foreign policy deals chiefly with Polish, Bohemian, and especially Venetian matters. The next chapter treats the issue of defense and analyzes the structure and social implications of Sigismund's important military reorganization after the defeat at Nicopolis (1396), the *militia portalis*, which was designed to defend against and wage war with the Ottoman Turks. Subsequent chapters analyze in detail the lesser nobility (whom Sigismund tried to strengthen as a counterbalance to the great barons), the middle class, the peasantry, the intelligentsia (that is, the clergy), and the king's relations with the church. The book concludes with a short treatment of general population trends and an extended analysis of culture in Hungary in this era.

The author is by no means uncritical of Sigismund and the implications of his reign for Hungarian history. Indeed, his detailed presentation reveals the weakening of royal power and the explosive social and economic tensions that afflicted Hungary. Mályusz does not gloss over personality and character weaknesses in Sigismund: the ten-page section (pp. 49–59) on Sigismund's person is especially effective. But, on balance, his portrait of the king and his role is more generous than previous scholars. In a revealing section (pp. 155–57) in which the younger king is compared with the older, Mályusz speaks of Sigismund's wisdom and prudence and the way he had mastered the art of ruling and had gained popularity.

Successful and impressive as this book is, it is not without flaws. Mályusz's thesis in chapter 5 that "the municipal population of the Anjou period developed into a [real] bourgeoisie in the first half of the fifteenth century" (p. 187) may be true (although it is probably overstated), but his treatment of this issue is in rather narrow economic and institutional terms. It does not fully develop the content of this urban outlook. Similarly, the chapter on culture concentrates more on mechanical matters than aesthetic and literary considerations. The dominance of courtly and chivalric culture is rightly emphasized, but, for example, there is no real assessment of the significance of Oswald von Wolkenstein's literary activity in Hungary. One cannot do everything in a single volume, however, even if it is the capstone of a career. This important study of Sigismund and his time is sure to be recognized as a standard work.

PAUL W. KNOLL
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N. L. PUSHKAREVA. *Zhenshchiny drevnei Rusi* [Women of Ancient Rus']. (Bibliotekhnaiia seria.) Moscow: Mysl'. 1989. Pp. 286. 1 r. 40 k.

According to N. L. Pushkareva's optimistic interpretation, women of medieval Russia enjoyed a wide range of possibilities for economic and political participation. In her view, women already enjoyed significant rights in the tenth century, when Grand Princess Ol'ga ruled as regent for her son Sviatoslav, and their situation improved steadily until the end of the fifteenth century, the chronological limit of the book. Pushkareva's interpretation contests a popular conception of Russian women's abject subjugation to their husbands in the seclusion of the *terem*, the women's quarter. The book provides a useful compendium of information and a synthesis of Russian and Soviet work to date, but it celebrates moments of female independence often without placing those inspiring exceptions in the grimmer context of most women's experiences.

This book belongs to the Soviet category of "scientific-popular" publications, intended for a wide audience. This undoubtedly helps to account for its shifting tone and eclectic organization. The book surveys many aspects of women's lives across six centuries in five loosely integrated chapters. Although Pushkareva attempts to discuss all classes, the overwhelming majority of evidence pertains to the wives and daughters of the princely elite. A lively "Gallery of Famous Russian Women" opens the book. The second and third chapters examine women's roles within the family and their legal status. Women's fashions occupy the fourth chapter, and the final chapter provides a historiographic review.

The introduction poses the central polemic of the work: were women competent, independent members of society, or were they secluded prisoners? This question addresses the central debate of the historiography, but the answers seem to lie beyond the chronological terminus of the book. Most recent work on Russian women accepts that only elite women were relegated to the *terem*, and then not until the sixteenth century. Because Pushkareva concludes her study in the late fifteenth century, it is not surprising that she finds little evidence of female seclusion in this period.

Coming out of the Soviet context, Pushkareva's work understandably lacks the latest conceptual and theoretical framework that one might expect in a Western work on women's history. Theoretical concerns aside, the book suffers from the uncritical outline of medieval Russian history that serves as background to its investigation of women. For instance, the book's extensive discussion of women's property rights ignores the current debate in Kievan history, initiated by I. Ia. Froianov, who maintains that large-scale landholding, male or female, did not exist in the early centuries of Kiev.

Nonetheless, in its exuberant chronicling of women owning land, appearing in court, acting as guardians, or expressing opinions on political affairs, Pushkareva's book makes an important contribution to the history of women and society in medieval Russia. She has gathered an astonishing number of shards of

evidence, all of which indicate that at least some women, primarily members of the princely elite, participated energetically in the economic and political lives of their time. Although no single chapter appears very persuasive on its own, in its proliferation of examples the book shows that some women could play an active role in medieval Kievan society.

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MODERN EUROPE

DAVID A. WEIR. *The Origins of the Federal Theology in Sixteenth-Century Reformation Thought*. New York: Clarendon Press of Oxford University Press. 1990. Pp. xi, 244. \$49.95.

Covenant theology has been of great interest to historians because of its importance for the Puritan background of Anglo-American culture as well as for the political theory of early modern Europe. David A. Weir, regarding federal theology as a "specific type" (p. 3) of covenant theology and defining it as adherence to a prelapsarian covenant of works alongside the earlier postlapsarian covenant of grace taught by John Calvin and other theologians, has made some interesting discoveries in the process of substantiating a thesis about the origins of double covenant federalism.

Weir's argument is that the emergence of the covenant of works arose from theological discussions in the Palatinate concerning Adam's fall and was first broached by the Heidelberg theologian Zacharias Ursinus in 1562. According to Weir, Ursinus's motive was that of theodicy, to offer a "milder" explanation of the fall than that offered by Theodore de Beza's supralapsarianism (pp. 63, 108). From Ursinus, Weir traces the double covenant to other reformed schools of the Palatinate and to the theologians Caspar Olevianus and Franciscus Junius. It was also picked up by the Puritan leader Thomas Cartwright, who was at Heidelberg in 1573–74, and by his protégé Dudley Fenner. After 1590, the two covenants became a standard point of Reformed theology and often its organizing principle. Double covenant federalism appeared in the *Irish Articles* of 1615 and *The Westminster Confession of Faith* of 1646. Weir claims that the covenant of works had significant ramifications: it provided a basis for human activities such as logic or science in a "covenant of works in creation" (p. 7), shifted piety to a concept of duty (p. 154), obliged believers to "work to make even the unregenerate obey the law of God" (p. 100), and meant that "the state could be entrusted with enforcing the law of God" (p. 6), including that concerning the Sabbath, on everyone. Other interesting points are made along the way: a passage in Augustine is cited as possibly the first appearance of a prelapsarian covenant of works; Sebastian Castellio, no friend of Calvin, is credited

with establishing *foedus* as a better translation of the biblical covenant than *testamentum*, a significant matter in the rise of federal theology; and the international character of Reformed theology is underlined.

Weir's book is a fine example of the genre of historical theology and, although primarily genetic in its description of theological development, it has much to teach Reformation historians. But two questions need further discussion. First, in his considerable sympathy for Perry Miller's approach to the covenant, which he describes as mistaken only in details (p. 16), Weir, although nuanced in his argument, perhaps makes too much of federal theology as a weakening or as an alternative to unconditional double predestination—a point at which Miller was mistaken in more than details. Double covenant and double predestination went together well in seventeenth-century Calvinism. Second, Weir's stress on the Palatinate discussions on the Fall as the single origin for the doubling of the covenant lacks compelling evidence and overlooks other factors. These other factors, such as Ramist dichotomizing, the needs of the spiritual life (at which Weir hints on page 108), contractual modes of thinking, anti-Pelagian emphasis on grace, and the impact of the Bible, did not all influence Ursinus, but they provided the larger context for the appearance and success of his idea. Weir specifically denies any role to anti-Pelagianism and biblical exegesis in the rise of the covenant of works, but the doubled covenant may have been a way of freeing grace from all taint of legalism, as Michael McGiffert has argued, as well as a way of organizing theology consonant with the historical character of the biblical record.

Weir's thorough and careful scholarship has produced an essential book for students of Reformation thought, especially for those who wrestle with the problem of the covenants.

DEWEY D. WALLACE, JR.
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LORRAINE DASTON. *Classical Probability in the Enlightenment*. Princeton: Princeton University Press. 1988. Pp. xviii, 423. \$49.50.

During the last thirty years, publication of works in the history of probability theory and statistics has constantly increased. In this respect the field can compete with the most research-active fields in the history of mathematics. Contributors to the history of stochastics, however, have different backgrounds and interests, and at least four groups can be identified: statisticians, probabilists, and historians of mathematics specializing in the history of stochastics, who are mainly concerned with the historical background of the mathematical concepts and tools of the subject; historians of science (and of mathematics), who try to unravel the intellectual conditions that influenced the solution of certain problems; philosophers of science,

interested in the historical roots of different concepts of probability or concepts such as determinism with a close connection to a certain understanding of probability; and historians of literature, who mine the riches of a probabilistic vocabulary in the literature of a given epoch. In addition, there is a sociological interpretation as exemplified by Donald MacKenzie's *Statistics in Britain, 1865–1930: The Social Construction of Scientific Knowledge* (1981).

Lorraine Daston falls into the second group, according to her own understanding as an intellectual historian writing about ideas. She labels her subject “classical probability”—borrowing the adjective from the common expression for literature and music of the same period. She locates its origins sometime in the second half of the seventeenth century and its end around 1840, and she is interested in the specific criteria characterizing classical probability as a distinct stage in the development of mathematical probability theory, distinguished, for example, from the calculus of probabilities in the second half of the nineteenth century.

For those interested primarily in mathematics, Daston provides an early warning that “the case of classical probability theory is less dramatic in a mathematical sense” (p. 5). Looking at the algebraical and analytical tools developed for probability theory by Abraham de Moivre and Pierre Simon Laplace in the eighteenth century, however, one might disagree. More important, a closer look at the mathematics of probability calculus could well provide substantial support for Daston's periodization. But Daston directs her efforts much more to general historians than to mathematicians or historians of mathematics. Therefore, she is more concerned with factors that allow the application of mathematics than with the technical aspects of such an application.

In the first chapter, Daston asks which qualitative notions of probability, distinguished by Ian Hacking (*The Emergence of Probability* [1975]) and Barbara J. Shapiro (*Probability and Certainty in Seventeenth-Century England* [1983]), became quantitative and why. Daston selects quasi-quantified legal concepts such as expectation and evidence as the most important factors in encouraging the quantification of certain aspects of the probable, and she treats the concept of quantitative expectation in close connection with probability. The understanding of probabilistic expectation, according to Daston, mirrored the “rational belief and action” of an elite represented by “l'homme éclairé” (p. 384). She depicts classical probability as a more or less mathematically codified description of rational behavior within this elite.

Two chapters deal with the theory and practice of risk and then with associationist psychology and its influence on the concept of the probable. Chapter 3, especially, is a gold mine of information on gambling and insurance practice in the eighteenth century. Daston describes the transition of the practice of risk from an antimathematical attitude to a readiness to

accept mathematical theory as a useful tool. This chapter will especially interest historians and sociologists, because Daston examines the origins of a mathematically oriented insurance system that has profoundly influenced the course of industrialized societies.

In chapter 4, Daston attempts to explain why classical probabilists did not distinguish between objective and subjective aspects of probability. The key factor for this apparent blurring between two different aspects of the probable Daston finds in associationist psychology. Marie-Jean-Antoine-Nicholas de Caritat (Marquis de Condorcet) and Laplace serve as witnesses for the influence of associationist psychology on their probabilistic work. The fifth chapter treats the attempts of enlightened probabilists such as Jakob Bernoulli, de Moivre, Thomas Bayes, Condorcet, Laplace, and Siméon-Denis Poisson to apply the urn model to all kinds of induction problems. It is followed by the story of probabilistic penetration into the moral sciences. The decline of classical probability is described in the epilogue in terms of a growing opposition to what were deemed unjustifiable intrusions into the realm of free will and of a shift from the ideal of *l'homme éclairé* to Adolphe Quetelet's *l'homme moyen*.

Daston's book is great fun to read because of its variety of well-chosen topics, thoughtfully interpreted and presented in wonderfully rich language. She extends considerably the range of subjects involved in the development of probability theory and so displays an impressive independence from conventional approaches to its history. Precisely because of the richness of her account, I especially regret her reservations against more mathematically oriented interpretations, especially because they might well complement her own arguments.

Be that as it may, the author's distinction between the legal roots of the quantification of probability and the result of this process should have helped her avoid repeating the standard Whiggish statement that “Pascal and Fermat invented the calculus of probabilities in their 1654 correspondence” (p. 23). Neither Blaise Pascal, whose discussion of probabilism is notorious, nor Pierre Fermat used the word “probability” or any French equivalent of it in their correspondence on games of chance. Yet the notion of probability is not subsumed within the concept of a contract based on equity as Daston assumes (p. 28). I would rather build on Daston's repeated statement that in the process of mathematization the original connotation of a concept is changed. With such a change one could explain the transition from a calculus based on legal expectation to an art of conjecturing based on probability and probabilistic expectation. Pascal's or Christiaan Huygens's understanding of expectation remained the legal or legally codified economic concept of expectation based on equity until Bernoulli, when mathematical expectation understood as the product of value and the

probability of achieving became available. Bernoulli's modification of Huygens's concept of expectation allowed him to identify it with probability in the sense of degree of certitude.

For the general historian this might appear a minor point. For the reader addressed by Daston, it counts much more that her book, with its rich historical tableau, thus provides a wonderful means for historians to overcome their anxieties regarding the history of science or the history of mathematics.

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RICHARD L. HILLS. *Power from Steam: A History of the Stationary Steam Engine*. New York: Cambridge University Press. 1989. Pp. xv, 338. \$80.00.

Contrary to the impression given by so many automobiles running on gasoline, the age of steam is far from over. It is steam that drives the turbogenerators that produce most of today's electric power, whether the heat comes from burning coal, oil, or splitting the atom. In this unpretentious book, evidently inspired by Francis Bacon's dictum that the "true office of history" is to "recount the events and relate the counsels," Richard L. Hills begins with the seventeenth-century antecedents. Then, drawing on his unique experience of two decades replicating and operating early engines at the Museum of Science and Industry in Manchester, England, he reconstructs the thinking of Thomas Newcomen and James Watt as they created the new species of prime mover that made history by converting the energy in fossil fuel to steam and then, for the first time, into mechanical work.

But the low-pressure engine, to which Watt remained just as devoted as Thomas Edison was later to direct current, was too cumbersome and inefficient to meet the power needs of nineteenth-century industrialization. Hills therefore rightly concentrates on the drive to take full advantage of the expansive power of steam by increasing pressures and decreasing the amount of steam admitted into the cylinder to push the piston. This meant more and more power supplied by less fuel in smaller engines. The price of this progress, which Watt had feared but Hills does not dwell on, was three decades of public alarm over horrifying explosions, until boilermakers and the Manchester Steam Users' Association made high-pressure steam relatively safe by 1890 through improved construction and rigorous inspection.

The eventual extinction of the reciprocating steam engine resulted from the limits inherent in its basic structure. Injecting externally produced steam into the cylinder, first on one side of the piston and then on the other at ever-increasing pressures, temperatures, and speeds, proved ever-more difficult, even with better materials, machining, and lubricants. Hills details the futile efforts of the old engine's devotees to

break through these limits, even as Charles A. Parsons was realizing the structural superiority of the much smaller turbine; it used steam in one direction and rotated with little friction at the high speeds so well suited for electric power generation.

Yet, as Hills makes clear, if the turbine prevailed in the early twentieth century, it owes its success to the art of steam generation and the science of thermodynamics and to the reciprocating engine that came before it. And, although obsolete for generating electricity, the piston and cylinder of Newcomen and Watt lived on in that most familiar of all fossil fuel heat engines: the achievement of the internal combustion engine's inventors was to remove the fuel from the furnace of the huge heat-wasting steam boiler and ignite small amounts of it inside the cylinder right under the piston. Indeed, internal combustion becomes historically intelligible only by reference to the external combustion engine that Hills follows from its origin to its demise.

Some may fault Hills for not venturing to explain why the English refused to use high-pressure steam long after the Americans enthusiastically adopted it, or why the French adopted the Woolf engine that remained unpopular in Britain, or why the turbine did not prevail earlier. Unfairly so, for Hills has stuck to his last and written a definitive technical history of the steam engine in Britain. To the general historian wishing to understand the technical mind, it would be difficult indeed to recommend a book that better reveals the genius of homo faber in the machine age.

CECIL O. SMITH, JR.
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CROSBIE SMITH and M. NORTON WISE. *Energy and Empire: A Biographical Study of Lord Kelvin*. New York: Cambridge University Press. 1989. Pp. xxvi, 866. \$89.50.

This new biography of William Thomson, Lord Kelvin, is written from the point of view of the importance of the Glasgow environment and practical, technological concerns for both Thomson's science and life. Crosbie Smith and M. Norton Wise claim in their preface that "Thomson's industrial vision thoroughly permeated his understanding of the natural world and the theoretical and experimental research which he pursued. . . . [W]e show that he drew extensively on conceptual and material resources available in his industrial culture and, with motivations structured by that culture, arrived at rational explanations of physical phenomena and at means of controlling those phenomena. Thus we seek to show, concretely and in detail, how the science that Thomson produced was inseparably integrated with the industrial culture that he represented" (pp. xx-xxi). This focus separates their work from Harold Sharlin's biography, *Lord Kelvin: The Dynamic Victorian* (1979). Sharlin's biography certainly did not neglect

technology and the technological culture of the time, but his focus was on "a great scientist who was also an engineer" (p. ix). This new, longer biography supplements Sharlin's work and adds a new emphasis. Through Thomson's life in Glasgow, we see the link between Victorian theoretical knowledge and the practical concerns that resulted in British world dominance in industrial production and maritime accomplishment.

The arrangement of the biography is thematic rather than strictly chronological to allow readers with interests in specific aspects of Thomson's varied career to find coherent thematic presentations. Each of the parts of the book has its own internal coherence. Part 1 develops the formation of Thomson's natural philosophy. Part 2 treats his work in mathematical physics. Part 3 investigates his many interests in the age of the sun and earth and in biology, geology, and cosmology. Part 4 deals with Thomson's engineering interests, especially his role in the Atlantic telegraph cable. Although the biography is a well-integrated whole, the separate sections do allow for pursuing special interests without the need to read the entire work.

The book is clearly a collaborative effort, showing the results of concern for integration. I found the writing to be clear, crisp, and of one voice. Even the few, more technical, sections that use some mathematical equations (for example, the discussion of Thomson's controversy with Maxwell over the nature of telegraph signals and light waves [chap. 13]), remain both interesting and informative. Smith claims primary responsibility for the sections on Thomson's early years, his system of the world, and his entrepreneurial activities. Wise is primarily responsible for the sections on mathematical physics. Both authors collaborated on the many other sections of the biography.

The unity of the collaborative effort of Smith and Wise seems to emerge, in part, from the broadly descriptive labels that the authors agree to use to describe aspects of Thomson's life. They employ labels such as "whig," "latitudinarian," "non-hypothetical," "anti-metaphysical," and "practical" when referring to various aspects of Thomson's life. These labels are well described by the authors. They help to form the image of Thomson that the authors wish to convey. For example, we find the "non-hypothetical" and "anti-metaphysical" Thomson avoiding metaphysics, mental constructs, and a priori knowledge in favor of direct experience and empirical evidence throughout his life. The term "practical" characterizes Thomson's life for the authors. He seemed to them to display an "all-pervasive spirit of engineering and industry" (p. xxi). The authors suggest that these labels "evoke the motivations which operated, sometimes implicitly and sometimes explicitly, in his everyday life" (p. xxi). The labels are persuasive and do help to form the character that the biographers wish us to appreciate.

The biography has a detailed and descriptive table of contents to guide the reader into the lengthy work. There is an extensive, up-to-date bibliography and a complete index. This biography deals with so many aspects of the technological and scientific culture of the late nineteenth century, in addition to the life of one of the century's most important figures, that all scholars of the period will find in it something of value. It is an excellent work of scholarship, so well written and presented that the reader runs the risk of reading the whole biography.

HENRY STEFFENS
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WAYNE THORPE. *"The Workers Themselves": Revolutionary Syndicalism and International Labour, 1913-1923*. (Studies in Social History.) Boston: Kluwer Academic and International Institute of Social History, Amsterdam. 1989. Pp. xviii, 352. \$83.00.

A considerable body of work exists both on the efforts of socialists from Marx to Lenin to unite workers internationally and on socialism and revolutionary syndicalism within European nations. The special merit of this important, meticulously researched, and excellent addition to the plethora of books on the working class is its focus on the international dimension of revolutionary syndicalism.

Proceeding chronologically, with a focus largely on the ideology of syndicalist labor leaders and on international syndicalist conferences and congresses, and using examples drawn primarily from France, Spain, Italy, the Netherlands, and Germany, Wayne Thorpe examines attempts by syndicalists to establish an international association of workers that would embody their particular principles. Rejecting reformism and political activity, the syndicalists strongly identified with the antiauthoritarian, anti-Marxist wing of the First International. Thorpe's tracing of a strong anti-revolutionary bias on the part of the French syndicalist Confédération Générale du Travail (CGT) at international labor conferences during the pre-World War I period (an attitude that paralleled the CGT's growing reformism within France) is particularly illuminating. The book provides a carefully documented analysis of the tensions and debates between the French and the more revolutionary non-French syndicalists over internationalism and how these eventually led to the creation by the First International Syndicalist Congress, meeting in London on September 27, 1913, of an International Syndicalist Information Bureau.

Early in the book Thorpe also lays the foundation for a major point of his conclusion, namely that the split between syndicalism and communism after the war had its roots not merely in the tactical shifts of worker organizations and parties responding to Lenin and the Third International but also in the deeply held syndicalist ideas concerning an authentic liber-

tarian worker-directed revolution. The heart of the book, impressive in its command of both the secondary literature cutting across national boundaries and all the relevant primary documentation, deals with the postwar era, which was characterized by the syndicalists' search for a genuine international and revolutionary path. Considering the thoroughness of this part of the work, the relative absence of any information on international syndicalist contacts and relations from the outbreak of World War I until the Russian revolution is surprising. International contacts among syndicalists (and socialists), albeit difficult, did continue during the war, and a summary of this line of development, even if not central to the story of the formation of a syndicalist international, would have made for a smoother narrative transition to the postwar period.

Turning to syndicalism in the years immediately following the Bolshevik revolution, Thorpe's book is a detailed and richly textured re-creation of the syndicalists' attempts to revive a revolutionary internationalist tradition within the European working class. The author is especially successful in conveying a sense of the Bolshevik revolution's powerful appeal and resonance for portions of the European working class. Thorpe's book contains a full account of the interaction between syndicalists and Bolsheviks (the genuinely revolutionary syndicalists never considered joining with reformist trade unionists who had gathered around the newly revived Amsterdam International), their theoretical and tactical differences, and, finally, the slow but growing awareness by the syndicalists that the Bolsheviks were inexorably moving in an authoritarian direction quite at odds with the syndicalists' initial expectations. This led the syndicalists to found in 1923 the International Working Men's Association, an international syndicalist organization far too long obscured from our view by the more illustrious Second and Third internationals. Thorpe's book is a ground-breaking study of syndicalist internationalism, an important addition to the literature of working-class internationalism in early twentieth-century Europe.

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AVNER OFFER. *The First World War: An Agrarian Interpretation*. New York: Clarendon Press of Oxford University Press. 1989. Pp. xix, 449. \$48.00.

This highly original and wide-ranging study presents a considerable challenge to the reviewer. Although Avner Offer initially claims to focus on the influence of food supplies on World War I, he ranges widely over the economies, politics, and social structures of several continents. The length of some of Offer's detours from his main argument—such as the three chapters devoted to opposition to Asian immigration

in the British settler colonies and the United States—will dismay some of his readers. In addition, having at the outset raised the issue of the true role of reason in the behavior of individuals, nations, and states, Offer never manages to reach a firm conclusion as to whether Britain and Germany could or should have behaved differently in the years before, during, and immediately after World War I. For all of that, the reader learns a great deal about British agriculture, the Atlantic economy of the Edwardian period, naval strategy, attempts to codify rules of naval blockade and neutrality before 1914, the politics of the British empire, and the fate of Germany during World War I, even if the significance of the information presented could certainly be clearer in many instances.

To emphasize the importance of the story he has to tell, Offer begins at the end rather than at the beginning and shows how severely food shortages eventually affected Germany's ability to carry on the war, especially in the crucial summer of 1918, which enabled the allies to bring about the armistice and impose the Versailles Treaty. He then turns to the origins of Britain's even greater dependence on overseas food and detours from there into an extensive discussion of British agriculture, showing how the need to support laborers, tenant farmers, and landowners outweighed the higher yields per acre in Britain and made competition with independent grain growers in the United States and Canada impossible. This in turn leads him to an analysis of British and North American social structures and to comments on the motives for emigration. Offer then argues that fear of Japan and of Asian immigration tightened the bonds between Canada, Australia, and the United States on the one hand and Britain on the other, although as his own evidence shows the British government did not really sympathize with exclusionist pressure and stuck to the Anglo-Japanese alliance despite the qualms of the dominion governments. Offer then turns to pre-1914 planning for economic war, focusing on the roles of Maurice Hankey, Sir John Fisher, and Viscount Esher, all of whom receive detailed treatment. Although Offer shows a fairly widespread belief in the importance of a blockade against Germany, he also admits that an opposing school believed in the need for military intervention on the Continent as well, and both strategies, of course, were implemented after the war began. The British government, in short, did not unanimously appreciate the decisive significance of the subject of Offer's book. Similarly, Offer exaggerates the influence of the growing Atlantic economy in explaining British diplomacy before 1914, ignoring other factors such as the need to maintain the ententes with France and Russia in order to safeguard positions in Africa, the Near East, and India.

Turning to Germany, Offer gives a guarded defense of the decision to build the high seas fleet, pointing out, quite correctly, that Germany was merely copying the other major powers in construct-

ing it and that Alfred von Tirpitz certainly did not favor an early war. Yet Offer waffles with respect to the influence of domestic-political factors on the German government's decision for war in July 1914, declining to reach a firm decision as have scholars such as Fischer and Berghahn, who believe those factors were critical, and scholars like myself, who discount them. Offer is fascinated by the issue of rational choice in history, and he tries to understand how the German government in 1916–17 could decide on unrestricted submarine warfare despite the logical weaknesses in the admiralty's case. He concludes that the ethos of the military, with its emphasis on the need for victory and its ingrained willingness to take risks, was responsible. In so doing, in my opinion, he slights another influence on operations during World War I, namely, the need, perceived by politicians at least as much as generals, to try any option that offered hope of decisive victory over one's opponents. (Critical postwar testimony by Theobald von Bethmann Hollweg, cited by Offer to prove his point [p. 365], seems to me to undermine his own case.)

A chapter on the shaping of the peace makes some questionable claims to strengthen the book's overall argument. Offer argues that Prime Minister William Morris Hughes of Australia, who certainly did try to wring the maximum benefit from his country's participation in the war, was principally responsible for the scale of Allied reparations demands on Germany. Another such claim is Offer's statement that Herbert Hoover, by forcing the Germans to spend gold on surplus American pork in the spring of 1919, "helped to precipitate the great inflation that followed" (p. 394). Both the harshness of the Versailles peace and the German resentment of it had causes other than the Allied blockade or the U.S.-British dominions alliance.

The work includes some other more minor mistakes. Bethmann Hollweg could hardly have explained "his restraint in Morocco" on "30 March 1911," because the second Moroccan crisis did not occur until later that year (p. 352). Nor, alas, is it correct to say that, although President Woodrow Wilson opposed Japanese demands for a declaration of racial equality in the League of Nations Covenant, he "was not a racist himself" (p. 213). The black organizations that supported Wilson in the election of 1912, only to watch as he segregated government buildings after coming to office, would rightfully have disagreed.

Offer gets back to the heart of the matter in his concluding chapter. The strength of the Atlantic economy and the political solidarity that partly grew out of it gave Britain a secure economic-strategic position and one that the Germans should never have challenged. (One may question his view that Germany might have been deterred by a more vocal assertion of Britain's position, however.) Yet, in the last pages, Offer makes another foray into counter-factual ra-

tionalism, asking whether the costs of British participation in the war were justified by the outcome. "Was there any other choice? The question has to be asked, even if it cannot be answered, if only to show how difficult it is to answer" (p. 408). Perhaps, but in the short run, I would argue, such questions distract us from the initial purpose of history, that is, determining why those now dead acted as they did. Both the Germans and the British—like governments today—were overwhelmed by forces that swamped rationality. We must take the time to try to understand what those forces were before we can establish the limits within which reason can play a role in decisions of peace and war.

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DENNIS E. SHOWALTER. *Tannenberg: Clash of Empires*. Hamden, Conn.: Archon, 1991. Pp. viii, 419. \$42.50.

The combat actions of the Russian and German armies in World War I have been under a cloud for most of this century: the former by the revolution, Soviet historiography, and the cold war, the latter by its involvement in the Nazi experience. The destruction of sources and the politicization of historians resulted in distorted images. In correcting this, two books stand out: Winston Churchill's *Unknown War* (1931) and Norman Stone's *Eastern Front 1914–1917* (1975). But neither includes more than a few dozen pages on this battle. Now Dennis E. Showalter's nicely balanced and readable volume fills in and corrects what earlier accounts left out or blurred. This book describes the battle of Tannenberg (August 17–30, 1914), a series of combat actions that resulted in the death, wounding, and capture of roughly 140,000 Russian troops in East Prussia at the start of World War I. The highlight of the discussion is contained in chapters 4–9, some of the better analytical narrative images of war in recent memory. The narration is framed by three introductory chapters, laying out the diplomacy, ideas, economics, and politics of the coming of the war, and two closing chapters, dealing with the battle's impact and aftermath, real and imagined. Fifty-one pages of notes, plus a brief essay, provide a thorough, critical bibliographical essay on sources, historians, and controversies.

Certain general strengths stand out. For both armies the author's integration of sources, including regimental and official histories, memoirs, and the massive secondary literature in both German and Russian, is masterful, and it is melded skillfully into a well-paced, occasionally riveting description of strategic, operational, and tactical actions. The relationship between technology and war is clearly drawn: for example, the influence (often for German forces) or absence (often for Russian forces) of railroads, air reconnaissance, telephone, and wireless interceptions on tactical and operational outcomes. The differing

combat ability of regular and reserve units, seldom discussed in military history, is made explicit.

Specifically, Showalter emphasizes that the Russian army was not nearly as bad as some accounts have suggested. He argues that its soldiers, field officers, and employment of artillery were at times more than a match for the Germans. It was poorly led at the high command level, poorly coordinated at the division and corps level, and many of its soldiers lacked certain modern mentalities—for example, a sense of time that might have enabled them to better coordinate attacks—which their opponents possessed. As for causation, the critically important Balkan Wars, 1912–13, are properly emphasized, and in the July crisis itself Germany is portrayed as the state that “did not slide into war in 1914 by accident or miscalculation. Instead it deliberately resigned the initiative to the game’s other players” (p. 93). It is worth comparing this with Samuel R. Williamson, Jr.’s statement in *Austria-Hungary and the Origins of the First World War* (1991): “Because of the linkages in European decision-making, this study on Austria-Hungary is perhaps fittingly focused on the one government that clearly initiated the violence in July 1914” (p. 1).

There is one problem. This book badly needs better maps than the four sketchy ones provided. Even for someone familiar with the battle and the East Prussian topography, following tactical and operational movements at the high level of specificity described here is sometimes difficult.

Overall, this book combines traditional battle narration with a “Delbrueckian” analysis of armies, society, and technology. Although this results in an occasional fracturing of the story and some loss of overall forward movement, there is no way around this intractable generic problem of military narrative. In the basement of the National Gallery in London are a series of great “battle canvases” by Horace Vernet, huge pieces roughly six feet tall and nine feet wide, capturing action in four French revolution and Napoleonic battles at crucial single points in time in such detail that individual units, commanders, and weapons may be readily identified. Also evident are dead and mutilated men and animals, clouds of gun smoke and dust, and not infrequently a storm, all suggesting Clausewitzian frictions. To analyze large-scale combat over a fifteen-day period with the technical detail of the Vernet paintings and still narrate and illustrate the frictions of war is a challenge to which this volume often rises.

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LENI YAHIL. *The Holocaust: The Fate of European Jewry, 1932–1945*. Translated by INA FRIEDMAN and HAYA GALAI. (Studies in Jewish History.) New York: Oxford University Press. 1990. Pp. xviii, 808. \$35.00.

In the history of the Holocaust, as with other subjects, great books usually develop important, overarching themes. In his pathbreaking *The Destruction of the European Jews* (1961), Raul Hilberg examined what he called the “machinery of destruction,” surveying the operation of the Nazi bureaucracy throughout German society and across a “semi-circular arc” of European states. Swimming through an ocean of Nazi documentation, Hilberg carried the reader stage-by-stage through his understanding of the killing process, from the definition of the Jews, to the confiscation of their property, to their concentration, and finally to their annihilation. In his authoritative look at the Jewish councils, or *Judenräte*, of occupied Poland, Isaiah Trunk (*Judenrat: The Jewish Councils in Eastern Europe under Nazi Occupation* [1972]) used Yiddish sources to present the world as the Jews themselves saw it, writing quite differently than Hilberg, who drew almost exclusively on German materials. To take another case, in his book on the Warsaw ghetto, Israel Gutman (*The Jews of Warsaw, 1939–1943* [1982]) placed the uprising within the pathological context of Jewish public life in Warsaw during the wartime period. Whether or not one agrees with these works, the reader always has a sense of a sure guiding hand, following a path defined by the interplay of sources and ideas.

No comparable focus or organizing principle animates Leni Yahil’s ambitious overview, with the result that few readers, I predict, will read the book in its entirety. This is not to say that the work lacks organization: if anything it is over-organized, with literally scores of divisions and subdivisions denoting issues, phases, geographic divisions, concrete examples, and so on. My sense, however, is that Yahil has been overwhelmed by the scope and complexity of her ghastly subject, with the result that the narrative bumps from one section to the next without much direction. Chapter and section introductions present new material rather than situating what follows, and passages that summarize or recapitulate are like oases in a desert. Following Yahil through eight hundred pages—even longer in the original Hebrew edition—is difficult as a result.

To be fair, the author sees her task as providing an “interim account” of Holocaust research rather than breaking new ground (p. 11). A senior Israeli historian and the author of an important study of the rescue of the Jews of Denmark, Yahil writes very much from the Israeli national perspective, but with a feeling for the universal significance of her subject. There is a good attempt to situate the Nazis’ assault on the Jews of Europe within the context of the war and the Nazis’ racial world view, although in her explanation anti-Semitism remains the consistent, driving force. Well within the “intentionalist” framework of historical explanation of how the “Final Solution” evolved, Yahil draws carefully on a body of evidence whenever making her case. She calls for a “rigorous scientific scrutiny” of evidence, and samples

primary sources abundantly throughout the work (p. 11). In this sense her survey is far more authoritative than those of Nora Levin (*The Holocaust* [1968]) or Lucy Dawidowicz (*The War against the Jews, 1933–1945* [1975]), who have written works of a comparable scale. Yahil's book is also much more comprehensive than other treatments of the subject. Her reach extends back to the history of modern anti-Semitism, ranges across Europe in her description of what happened to the Jews in wartime, and encompasses full discussions of such matters as the different stages of Nazi policy toward Jews, negotiations with the Germans, Allied policy, bystanders, and so on.

Because of the diffuse quality of the book, readers will most likely refer to it occasionally for its discussion of specific themes. Most will find it particularly useful for its distillation of a vast literature on East European Jewish communities during the Holocaust—memoirs, theses, documentary collections, and histories of particular communities—available mainly in Hebrew, through the diligent activity of research institutes and individuals in Israel. Drawing on such material, for example, Yahil presents a detailed description of the tyrannical Jewish leadership of Moshe Merin in Zagłębie, in eastern Upper Silesia, and a discussion of the ghetto of Piotrków Trybunalski in the Generalgouvernement where, to the contrary, the ghetto leadership managed for a time to protect the Jewish community. I know of very little written on these subjects outside Israel. There is as well extensive discussion of themes referred to in the English language or European literature but for which Israeli writers and Hebrew language sources provide crucial additional evidence and important interpretations. Examples include: rescue operations, debates within Jewish communities, Jewish religious life, concentration camps, and the extermination process. Israeli writing has addressed themes such as these to a greater degree than elsewhere, and through Yahil's book much of this material is presented to the general reader in English for the first time. The book also includes a separate chapter by Gutman on armed Jewish resistance in Nazi-occupied countries, accenting the massive assault from Israeli quarters on the "sheep-to-the-slaughter" interpretation advanced by Hilberg and Hannah Arendt.

Yahil reminds us of the horrors of the Holocaust, infusing with feeling a text that is both reliable and well documented. More than this, she communicates to the nonspecialist what a huge panorama was the massacre of European Jewry and how much effort has gone into trying to understand what happened.

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JONATHAN STEINBERG, *All or Nothing: The Axis and the Holocaust, 1941–1943*. New York: Routledge. 1990. Pp. xiv, 320. \$27.50.

Jonathan Steinberg asks why highly placed Italian diplomatic and military officials defied their government and the Germans to protect the Jews who were under their control from 1942 to mid-1943. His book is divided into two parts. The first traces the decisions taken by Italians such as Giuseppe Bastianini, the governor of Dalmatia and later foreign minister, diplomats Count Luca Pietromarchi and Roberto Ducci, and generals Mario Roatta and Giuseppe Pièche. The second part probes deeper reasons for the different behavior patterns of comparable German and Italian officials.

Two events forced the Italians' hand. By mid-1942 Italians in the Balkans understood the nature of the Nazi "Final Solution." Then, on August 21, 1942, Mussolini approved the German request to hand over the Jews of Croatia. Steinberg rejects easy answers to explain why the Italians thwarted orders of the Duce and the desires of the Germans: "The remarkable national resistance took place in the face of a great evil and was carried out by people who were often precisely the same people who interned and tortured innocent Slovene and Croatian civilians. They agreed out of a mixture of horror, humanity, prestige, sense of honor, military necessity and self-interest that there was a border beyond which they could not and would not go" (p. 133). The Italians argued that their relations with the local Serbian *chetnik* militia would be destroyed if another protected group was handed over to the Germans. Moreover, they were aware that Italy had lost the war and did not want the Holocaust on their record. But self-interest only clarifies so much. Steinberg argues that the Italians pulled back because of a deeply held sense of humanity, whereas German records reveal only one case of an official who raised the claims of humanity in dealing with the Jews. Tragically, many of those who were involved at the highest levels in the German military and diplomatic service were anti-Nazi but did not disobey orders because they deemed them formally legitimate.

Steinberg is quite convincing about the practical differences between the fascist and Nazi systems. The disorganization that plagued the fascist war effort played into the hands of those who wished to defy their superiors. But Steinberg's analysis is less satisfying when he passes to the more speculative second part. His chapters on the differences between the Nazi and fascist regimes, leadership cadres, and armies are well done, but they add little to what we already know. Steinberg might have been wise to stop short of philosophical generalizations about the nature of Italians and Germans. Inevitably, he leaves the impression of a moral determinism that drew the Germans to the "Final Solution." If one concentrated on the thugs of the Italian Social Republic, the Italians would not look quite as heroic.

For the historian of fascism Steinberg's fine book helps answer a troubling question. After 1938 many intelligent fascists understood the dangers of Musso-

lini's foreign and domestic policies. During World War II they vainly tried to defend the Italian war and its sphere of influence. But what might they have done when confronted by Italian collapse and a German victory? Bastianini, Roatta, Geloso, Pietromarchi, and the others confronted just such choices in starkly human terms. Steinberg intelligently balances the moral and political bankruptcy of the best of the fascist elite against a noble, but belated, attempt at redemption. In the end, the Italian refusal to participate in the "Final Solution" stands out, even though the fall of the fascist regime led to the seizure of many of the very same French and Balkan Jews whom the Italians protected.

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LUTZ KÖLLNER *et al.* *Anfänge westdeutscher Sicherheitspolitik, 1945–1956*. Volume 2, *Die EVG-Phase*. Munich: R. Oldenbourg. 1990. Pp. xxvi, 914. DM 98.

In August 1954, the French Assembly voted not to ratify the European Defense Community (EDC) Treaty, which provided for the establishment of a "European army." With this decision France aborted the birth of what many Europeans (and Americans) saw as a great hope for a new united Europe, but which in reality would undoubtedly have been a premature—and probably severely handicapped—creature. Yet the fact that the ill-conceived EDC never saw the light of day does not mean that the four-year effort to produce it was entirely wasted or without lasting significance. The main purpose behind the EDC project, after all, was to provide a framework in which West Germany could be simultaneously armed and controlled. A common European army that included the Federal Republic would supposedly help the Western alliance deter Soviet aggression while preventing the Germans from reviving any aggressive designs of their own. This remained the guiding idea behind West Germany's integration into NATO, which was effected soon after the collapse of the EDC.

This book, volume two of the *Militär-geschichtliches Forschungsamt's* (MGFA) projected four-volume *Anfänge westdeutscher Sicherheitspolitik, 1945–1956*, is an exhaustive (and at times exhausting) examination of the effort to arm the Federal Republic of Germany via the European army concept. Like the first volume in the series, which traced the development from postwar "demilitarization" to the decision by the Western powers to try to get "their" Germans back into uniform (albeit not *feldgrau*), this volume adopts a multifaceted approach, embracing the international, domestic, organizational, and financial aspects of the problem. Each of these subcategories is analyzed in a separate section by one or two of the four MGFA historians assigned to the project. All the contributors had access to newly opened German,

British, and American archival collections (though lamentably not to French and Soviet files). The result is by far the most comprehensive and thoroughgoing study now available on this crucial phase of the German rearmament process—and indeed on the brief life of the EDC.

In part one, Klaus A. Maier does an excellent job of clarifying the complicated international context in which the EDC concept was born and died. He investigates the American, British, and West German conversion from their original preference for the Federal Republic's direct integration into NATO to the European army plan, which was launched by the French as a way of controlling and circumscribing the apparently inevitable rearmament of their feared neighbor to the east. But, as Maier correctly argues, the EDC plan was rife with fundamental flaws. In the first place, it put the cart before the horse, trying to establish military integration before common European political institutions had evolved. Although the British went along with the Americans in backing the plan, they would not themselves join the EDC, for such a binding commitment to the continent would have further weakened the British imperial role and claims to world power status. Britain's refusal to join the EDC undermined the concept's usefulness as a check on the Germans, which of course alarmed the French. France's enthusiasm for this enterprise was further diluted by West Germany's ability to eliminate through negotiation some of the original plan's discriminatory controls on the German contingent. Nor did it help that America's shift to its "New Look" strategy of reliance on nuclear weapons discredited the EDC both by making it look like a possible replacement for U.S. troops in Europe and, even worse, like little more than a multinational pile of nuclear cannon fodder. In response to these developments, France decided to pursue its own nuclear armament as an alternative to the EDC, which in any case had been criticized in France as a threat to its sovereignty. Yet the much-lamented collapse of the EDC, Maier notes, amounted only to a brief setback for the German rearmament effort because those responsible for implementing the subsequent "NATO solution" could fall back on the extensive spadework done by the EDC planners.

The domestic political forces helping to shape West German security policy in the EDC period are ably discussed in part two of this volume by Hans-Erich Volkmann. Dominating the domestic scene was the ongoing struggle between the government of Konrad Adenauer, which eventually pushed hard for the EDC despite objections to its discriminatory qualities, and the Social Democratic Party (SPD), which continued to oppose any rearmament scheme that would jeopardize German reunification. The tensions between Adenauer's security policy and aspirations for German unity came to the fore when Josef Stalin delivered his famous "note" of March 1952, which essentially offered reunification and limited rearma-

ment in exchange for German neutrality. While the SPD, and even elements in the coalition parties, found this offer attractive, Adenauer rejected it out of hand, offering as an alternative his *Politik der Stärke*, which insisted that Western strength and unity alone could force the Soviets to relinquish control over East Germany and ultimately over all of Eastern Europe. Adenauer's and the Western powers' curt dismissal of the Soviet initiative led many Germans to speak of a "missed chance" for German reunification. Recent events notwithstanding, many continue to believe that Adenauer's policy was misconceived. Yet Volkmann is certainly correct to remind us that German neutrality was no more acceptable to the world community in 1952 than it would be in 1990. In any event, Adenauer survived this and other challenges to his foreign policy and managed to force the EDC Treaty through the legislative process. Volkmann shows that in doing so the chancellor significantly clarified the power relationships between the various branches of government.

In part three, Wilhelm Meier-Dörnberg skillfully leads the reader through the maze of negotiations surrounding the EDC Treaty. In these the demand by West Germany for military "equality" had to be reconciled with the Allied (not just French) determination to keep the projected West German contingent on a tight leash. The collapse of the EDC ultimately gave the Federal Republic more freedom to develop its new army as it saw fit, although the NATO solution retained some of the controls envisaged in the EDC.

The final section, a discussion of the financial aspects of the EDC by Volkmann and Lutz Köllner, is especially welcome because few histories of the rearmament question pay much attention to this issue. Yet here, too, the EDC era was crucial, setting the pattern for many later battles over who would pay how much for security. In this domain the West Germans were not so anxious to be accorded full equality.

Although the authors of this volume do not seem to differ significantly in their points of view (something one cannot say for all MGFA "team" projects), it would have been helpful to include a concluding chapter that pulled the central arguments together and perhaps also clarified the many ways in which the security questions of the early 1950s are relevant to those of today. To what degree might that premature creature of 1954, the EDC, be seen as a model for the 1990s?

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NICHOLAS HOWE. *Migration and Mythmaking in Anglo-Saxon England*. New Haven: Yale University Press. 1989. Pp. xiii, 198. \$25.00.

Nicholas Howe's book is not a study of the historic coming of the Angles and the Saxons to England but instead an examination of the impact of migration on the Anglo-Saxon imagination. As a result of his investigations, the author gives a thoughtful and original interpretation of some of the central texts of the pre-Conquest period: Gildas's *De excidio Britanniae*, Bede's *Ecclesiastical History*, *Browulf*, and a number of other vernacular poems. As a discerning literary critic, the author alerts us to the fictions and myths incorporated into the work of the most sober historians of the early Middle Ages. His discussion of the Old English version of Bede's story of the migration is good, and his section on the language of Exodus and its vocabulary of the sea is brilliant.

As interesting and stimulating as this book is, however, it is bristling with opinions that historians will find difficult to accept. It is doubtful, for example, that Archbishop Wulfstan's diocese was "populated largely by heathen Danes" (p. 10), that Wulfstan only knew Gildas's work through a letter of Alcuin and not through Bede (p. 20), or that the *Anglo-Saxon Chronicle*, "because it is most constrained by chronological exactitude, seems most resistant to myth" (p. 29). This is the chronicle, after all, that describes for us Kent's settlement by equine brothers and King Alfred's descent through Woden. Because the author examines Gildas and Bede only in the context of native descriptions of migration and not in the company of other historians, he ascribes to them innovations that were clearly not of their making. Howe, for example, believes that the greatest contribution of these two authors toward the shaping of the English migration myth was their replacing of chronology with geography as the main structure of their narrative. The insertion of geography into historical writing, however, was a thousand years old by the time Gildas took up his pen. Howe also believes that Bede's lack of dating in his narration of the years 449 to 597 was an act of will and that Bede abandoned chronology in his telling of the English migration because he recognized that a dateless format was a better one for myth making (p. 69). Howe seems to suggest that a proof for Bede's self-conscious shunning of dates during these years is the *Anglo-Saxon Chronicle's* use of dates during these same years (p. 69). This argument, however, is not convincing. The *Chronicle* was dependent on Bede for much of its early information. The chronicler took what he could from the *Ecclesiastical History* and then organized this information into a chronological framework. He did this because the *Chronicle* was an annal, and annals cannot be written in any other fashion. Bede was personally interested in time, in figuring, and in chronology. His history presents not one but two dates for the *adventus Saxonum*, suggesting that although he was working hard to place this most important of events within the framework of Christian and universal history, he was unable to do so.

Despite these difficulties, Howe's book is provoca-

tive, interesting, and beautifully written. Most important, it serves as a salutary reminder for those of us who work with these texts as historical documents rather than literary artifacts that myth and imagination, as well as a retelling of the past, were central in the creation of these texts.

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DAMIAN RIEHL LEADER. *A History of the University of Cambridge*. Volume 1, *The University to 1546*. New York: Cambridge University Press. 1988. Pp. xxi, 399.

This book is the first of a projected multivolume history of the University of Cambridge, conceived in self-acknowledged response to "the launching of a mighty dreadnought in Oxford," the multivolume *History of the University of Oxford* (p. xiv). The University of Cambridge Press plans four single-authored volumes and began only when Damian Riehl Leader presented the press with a volume that could serve as a first installment.

The single-author approach has much to recommend it: unity of style, better balance, and unity of vision, which Leader has in fact achieved. His volume combines institutional structure with the content of learning and blends a topical approach with adequate chronological development.

Leader brings together much of the research of the last generation. He also draws from his own work on the teaching reforms in fifteenth- and sixteenth-century Cambridge and attempts to distill intellectual activity from surviving booklists of college and convent libraries. He has written the only recent survey of the first three centuries of Cambridge academic life and has corrected much of the older scholarship in light of recent research. The institutional and curricular chapters are particularly sound.

The major problem with the book lies in the description of Cambridge's contribution to late medieval philosophy and theology. The section on logic, for example, is basically a partially informed description of the late medieval English curriculum based largely on Oxford evidence, and insufficient attention is given to the small but important body of Cambridge texts uncovered by L. M. De Rijk and J. Ashworth. The discussions of school traditions, especially the conflicting approaches of realism and nominalism (pp. 124–25, 130–31, 158, 182), are simply stereotypes reintroduced from the older literature.

Even less satisfactory is the chapter on the theological faculty, which combines traditional assessments with evidence derived from late medieval booklists and uses A. B. Emden's *Cambridge Register* to support most assertions about intellectual content (see especially pages 178 and 180). One is told that Cambridge had almost no distinguished theologians and was at best a pale reflection of Oxford until the late

fifteenth century, but that truism is not tested by an examination of sources long available nor by the inclusion of important modern research. There is no mention, let alone study, of the extant *Sentences* commentaries of Thomas Ringstead or John Walsham from fourteenth-century Cambridge nor the Norwich disputation of Bartholomew of Reppes, who was almost certainly a product of Cambridge. Nor have the extant biblical commentaries of such Cambridge masters as Henry Costesey, Thomas Hopeman, William d'Eyncourt (barely mentioned on page 187), or Thomas Ringstead been explored, although portions of these works were discussed by Beryl Smalley in *The Study of the Bible in the Middle Ages* (1941) and *English Friars and Antiquity in the Early Fourteenth Century* (1960) (the latter appears not to have been consulted at all). Use has not been made of the cited opinions of other fourteenth-century Cambridge theologians, such as Walter Beaufon or Ralph Pigaz.

A few additional corrections need to be noted. Lectures on the *Sentences*, which were given by bachelors of theology, should not be described as "regent lectures" (p. 57). Scotellus is a name normally assigned to Peter of Aquila and less frequently to Antonius Andreas (p. 158). It would have been well to have avoided nontraditional spellings: for example, Francis de Meyronibus for Francis de Meyronnes (pp. 180–81) or Thomas Bokyngham for Thomas Buckingham (p. 180). The discussion of Thomas Sutton's critique of Robert Cowton (p. 180) is based on the false assumption that the *Sentences* commentary attributed to Thomas Anglicus is by Sutton. Robert Holcot's works were never condemned, let alone for "Ockhamist tendencies" (p. 186). The presumption that regent lectures died out as printing came in (p. 246) does not consider that such lectures were, for almost two hundred years before printing in England, largely composed of commentary and questions, not line-by-line analysis of a text. Finally, some notice should have been taken of R. N. Swanson's *Universities, Academics, and the Great Schism* (1979) as well as his "Universities, Graduates, and Benefices in Later Medieval England" (*Past and Present* [1985]), both of which apply to Cambridge as well as to other universities.

It is said that competition is healthy, but surely only with adequate preparation and forethought. The author should have been encouraged to make his study definitive. As a reasonably balanced survey of the known history of the early University of Cambridge, the book is useful. As the first volume in a new and presumably newly researched history of that university, it does not do justice to the strength and vitality of late medieval Cambridge. It is hoped that subsequent volumes in this series will better fulfill expectations.

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JONATHAN GOLDBERG. *Writing Matter: From the Hands of the English Renaissance*. Stanford: Stanford University Press. 1990. Pp. xiv, 349. \$29.50.

This is an odd book that traditional historians may find baffling. It examines the technical manuals and copy books of English and Continental writing masters from the sixteenth and seventeenth centuries in order to reflect on the origins of modern subjectivity and, ultimately, "the closure of modernity" (p. 281). Jonathan Goldberg claims to write "from the perspective of the politics of the cyborg" (p. 12), which is represented as antihumanist feminist socialism. His closing words are partly a joke: "a machine wrote this" (p. 318). At least we have been warned.

The Library of Congress classifies this book as a history of paleography, penmanship, manuscripts, and writing in Renaissance England. But its real business is Derridean grammatology and the deconstruction of logocentrism. Goldberg acknowledges that his work does not operate within the "usual descriptive, documentary mode" of paleographers and historians. It depends, rather, on the theories of Jacques Derrida and the Derridean vocabulary of "cultural graphology" (p. 2). Derrida is present throughout this book and is joined by Friedrich Engels, Sigmund Freud, Martin Heidegger, and Roland Barthes in a conclusion that verges on gibberish.

We read here of the hand "making itself within the self-simulation that defines a being-in-the-hand stripped of ontological security" (p. 55); of "the aporia of empowerment of and by letters, the enabling and disabling recursivity in which the subject of writing is written into the regulatory domains of differentiated scripts" (p. 164); and "deauthenticating authentication that allows the circulation of power always to be in another hand" (p. 264). Toward the end Goldberg glosses a passage by Heidegger as "not immediately elucidatory" (p. 294). Readers of this book may experience similar problems.

Perseverance, however, unwraps a range of stimulating observations. Goldberg provides an insightful account of the materiality of writing, based on close viewing of woodcuts and frontispieces as well as close reading of printed texts. The hand writes, and the hand is written. Pen and penknife combine to cut, shave, or plough the page. Script and apparatus are in perpetual tension. But whether the pen is phallic (p. 99) or its action violent and generative (pp. 74, 178) remains open to question.

Goldberg invokes a world of secretaries, scribes, clerks, notaries, scriveners, writing masters, and their patrons and clients, mastering and promoting the new technicalities of literacy. He shows us self-advancing humanist pedagogues (such as Richard Mulcaster and Roger Ascham) negotiating their position within structures of state power. In the hands (literally) of tutors and secretaries, "humanistic pedagogy circumscribes itself so that it can inscribe the world" (p. 114). Literacy, in this reading, was inseparable

from domination. Even the letters of the alphabet were shaded by structures of authority.

Nor were the multiple scripts of Renaissance Europe free from hierarchical implications. "The skirmish . . . over secretary and italic script plays out an ideological and class battle" (p. 119), italic being the hand of noble refinement and also, in England, of centralized monarchy (p. 52). Much of this has been said before by historians of handwriting, but Goldberg politicizes and reenergizes the discussion. Although minutely attentive to the penmanship discussed and displayed in the writing manuals, this book shows little evidence of exposure to holograph manuscripts in the archives. It is more about scribal theory than scribal practice.

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SUSAN BRIGDEN. *London and the Reformation*. New York: Clarendon Press of Oxford University Press. 1989. Pp. xix, 676. \$96.00.

Susan Brigden has written a highly detailed narrative of the complex movements that constituted the Reformation in early and mid-Tudor London. In many ways this is the most remarkable of a number of local studies of the English Reformation that have appeared over the last twenty-five years. The author has, of course, benefited from the refinements of interpretation that historians such as A. G. Dickens and Christopher Haigh have made concerning the distinctions between the official and popular reformations and whether the impetus for reform and evangelization came from native popular religious movements, official religious settlements, or foreign influences. Previous studies of other counties and dioceses in England and Wales have tended to stress how slowly the Protestant Reformation proceeded in most parts of the hinterland, but, lacking a modern study, scholars have assumed that the London Reformation was a fast reformation. What we now learn is that the reformation in London proceeded more slowly than anyone could have guessed and was but half-finished by the accession of Elizabeth in 1558, when this study ends. One can only hope that Brigden will finish the story with another volume on the reign of Elizabeth.

Regarding the alteration of religion, London was in many ways the theater of England, and Brigden skillfully interweaves an analysis of popular piety with accounts of theological controversies and discussions of "high politics." National politics and London politics were at times during this period "indistinguishable" (p. 522). Brigden's effective and extensive use of wills not only reveals the religious complexion of London during the spread of popular Protestantism but also shows how their bequests and patterns of giving helped shape popular devotions, preaching,

philanthropy, and education. We learn that the anticlericalism of Londoners on the eve of the Reformation has been exaggerated and that half of all Londoners owning substantial property still made bequests to religious houses, especially those of the Carthusians and Observants. There was much popular support for religious reform flowing through the channels of orthodoxy. Most citizens were scrupulously orthodox in the 1520s, and between 1529 and 1546 only 10 percent of the preambles of London wills can be classified as evangelical, compared with the 77 percent that can be described as Catholic.

A small but very well organized and closely related Protestant movement began to emerge, and, by the late 1520s, the Lollards began to interact with intellectuals of evangelical persuasion. The author also provides much evidence of how closely Thomas Cromwell was linked to the evangelicals and how shamelessly he manipulated London parliamentary and municipal elections. Conservative Londoners were numerous enough to make trouble, but the royal commissioners compelled just about every Londoner from the mayors and aldermen to the lowliest of apprentices and servants to swear oaths to the Henrician supremacy. Only one of the conservative parish clergymen refused to swear the oath. The conservative ecclesiastical authorities forfeited much lay support by overreacting to the threat of heresy, and clerical denunciations of laymen for heresy, often linked to tithe disputes, increased popular fear of ecclesiastical authority and made people look to the king for protection against a power even more arbitrary and dangerous than his. There were parishes that became distinctly Protestant early on and others that remained steadfastly Catholic, at least through Henry VIII's reign. A map would have helped readers visualize these patterns, and a few more tables would have made it easier to digest the enormous amount of information contained in this excellent book.

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KASPAR VON GREYERZ. *Vorsehungsglaube und Kosmologie: Studien zu englischen Selbstzeugnissen des 17. Jahrhunderts*. (Publications of the German Historical Institute, London, number 25.) Göttingen: Vandenhoeck und Ruprecht. 1990. Pp. 222. DM 62.

Students of the religious personality have always found Puritan diaries both fascinating and puzzling: revealing so much, they seem to conceal even more. Kaspar von Greyerz, a Swiss early modernist, has looked closely at some sixty English autobiographical writings to discover what they tell us about expressions of piety among Calvinists and other Nonconformists in the seventeenth century. His book, which originated as a *Habilitationsschrift*, is presented as an endeavor in the social history of religion; in fact, it is

a rather old-fashioned mentality study proceeding along traditional lines of content analysis toward conclusions that largely confirm what we have long assumed about the Puritan way of religion.

A useful comparison with German and Swiss autobiographies shows the English diary to be a relatively late development: only two are known to have been written in the fifteenth century, and a mere six in the sixteenth century. But, beginning with John Dee's diary, which covers the years 1554 to 1601, the characteristic feature of English autobiographical writing in the confessional age was in place: concentration on the inner person of the author rather than on the outward events of his or her life. In its two types, that of the spiritual autobiography, in which conversion is at the center, and that of the spiritual diary, which records a process of religious self-disciplining, the genre was specific to Protestants (although a comparison with the state-of-one's-soul reports demanded of the laity by Jesuits in seventeenth-century France, as recently investigated by Louis Châtellier in his *L'Europe des dévots* [1987], might have modified this judgment) and to the middle and upper classes, whose advanced literacy served them as a vehicle of self-presentation. Von Greyerz shows that the object of writing was two-fold. Most diaries and autobiographies being intended for publication, they portrayed the writer as a witness to God's crucial intervention in daily life, thus offering proof of the workings of Special Providence. More importantly, they served their authors as an instrument for coping with the emotional pressure exerted by a stern religion: by registering signs of likely election, they assuaged natural doubts engendered by intimations of the limited availability of salvation according to the doctrine of double predestination. In this way, providentialism helped diarists to demystify God's dispositions by seeing them made concrete in the life of the individual. No longer a terrifying enigma, God became a friendly force offering providential grace to all who proved themselves just.

In tracing his samples of "self-witnessing" over three-quarters of a century, von Greyerz finds that the consensus that had produced a common providential cosmology by the end of the sixteenth century began to splinter after the middle of the seventeenth century. A cluster of causes was responsible for this disintegration, chief among them the Civil War, the influence of the mechanical philosophy among the literate, and important changes occurring in the law. Diaries recorded this transition: toward the end of the century, few diarists saw illness as a divine punishment; many grew critical of witch persecutions; and there was evidence even of elements of Deism. Von Greyerz deals with this important transformation in rather summary fashion; a more developed analysis of the change would have been welcome.

One would also have appreciated some consideration given to the textual difficulty of interpreting Puritan diaries and to their social situation in middle-

class religiosity. It is obvious that religious self-representation had by 1600 come to take on a fixed literary form that must have exerted its own demands on writers. To what extent, then, may one take these works as direct clues to particular states of mind? Again, what functional importance did autobiographical writing have in the development of middle-class consciousness? More than 300 English diaries are known from the period, and nearly 100 formal autobiographies. Does religious need alone explain this outburst of "self-witnessing," or were other factors also at work? Von Greyerz is aware of these problems. But he does not take the time to explore them. It would be interesting to see his work extended in a more critical and social-historical direction.

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FELICITY HEAL. *Hospitality in Early Modern England*. (Oxford Studies in Social History.) New York: Clarendon Press of Oxford University Press. 1990. Pp. x, 452.

The tests of a good historical monograph are its ability to combine originality, conceptual sophistication, historical insight, and scholarly depth. On all these counts, Felicity Heal's new book on hospitality scores highly. Not only has Heal identified a crucial theme in the social history of early modern England that has scarcely been looked at before but she explores it through a range of sources and with a breadth of erudition that is truly impressive. The book examines how the ideal and practice of hospitality changed in England between about 1400 and 1700, looking not just at the landed elite but also at the clergy, town-dwellers, and the population at large. Care is taken to set the findings in a broad comparative and anthropological context, with the author offering shrewd (though never obtrusive) contrasts with the Celtic fringe, continental Europe, Tidewater Virginia, and even the bedouin culture of Egypt.

A number of general conclusions emerge from this study. In England the ethos of generosity and public largess was most powerful among the aristocracy and survived largely intact well beyond the early modern period. Early modern English men and women, however, appear never to have been particularly hospitable toward outsiders or strangers, an attitude that changed little throughout the period under consideration. The most dramatic change over time can be detected in the area of the duty of hospitality toward the needy. The experience of economic and demographic crisis in the sixteenth century, the changing attitudes wrought by the Protestant Reformation, and the growth of state provision for the poor led to a decline in indiscriminate almsgiving, as beneficence

came to be shaped by more prudential calculations about the effects of giving and the moral worthiness of the recipient. But such generalizations do an injustice both to the subtlety and to the range of arguments contained in this book. Heal shows herself to be particularly sensitive to questions of continuity and change, as well as to the exceptions to the general trend, as can be illustrated from her discussion of the impact of the Reformation on attitudes toward material charity. Thus, we learn that whatever changes the Reformation might have effected in the long term, the early Protestant writers actually helped revitalize the idea of hospitality in the mid-sixteenth century. Moreover, although it might generally seem true that Puritan clerics were less generous than their Anglican counterparts, there are counter examples of Calvinist saints stressing the importance of material charity, while Archbishop William Laud was not only notoriously ungenerous but his lack of generosity was even defended by his high Anglican apologists.

A number of important sub-themes, familiar to all students and teachers of early modern English social history, are examined from a fresh perspective. In addition to the debate over the social consequences of the dissolution of the monasteries, Heal offers much insight into the questions of poverty and vagrancy, urban rituals, the celebration of rites of passage, and gender issues. Even witchcraft makes a brief appearance. The author is perhaps overcautious in drawing her conclusions; the perpetual qualifications make the argument difficult to follow at times. More focused discussions of some of the broader implications of her findings would have helped. For example, the issue of social control is a central underlying theme in the book but is never explored directly; merely providing different examples in diverse parts of a long book showing that the elite could be both willing and reluctant to use hospitality as a form of social control is somewhat unsatisfactory. But any book as original and inventive as this is bound to raise as many questions as it answers, and Heal must be congratulated for providing a masterful study that will inevitably prove to be a powerful stimulus to future research.

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MAURICE LEE, JR. *Great Britain's Solomon: James VI and I in His Three Kingdoms*. Urbana: University of Illinois Press. 1990. Pp. xvi, 332. \$32.50.

Until fairly recently, historians of England have normally portrayed James VI and I as a king whose slovenly manners, weakness for favorites, extravagance, appeasement of Spain, and inability to get along with Parliament fueled the rise of opposition. Although in the past two decades a number of revisionist articles have repeatedly challenged this nega-

tive view, the absence of an adequate book-length study, capable of supplanting D. H. Willson's unsympathetic biography of 1956, has made it difficult to bring James's complex career into focus. This balanced and solid narrative by Maurice Lee, Jr., goes a long way toward filling this gap.

The book is at its best in four opening chapters that place James's early career within the context of Scottish politics. Lee shows how James's intellectual formation grew out of circumstances created by the Scottish Reformation, which he regards as a much weaker movement than historians have usually assumed. Political weakness encouraged aggressive efforts to subordinate royal authority to the agenda of the reformers. The young king was subjected to rigorous indoctrination in Calvinist theology and constitutionalist theory at the hands of his formidable tutor, George Buchanan, and later to aggressive lectures by Presbyterian clergy such as James Melville. James absorbed the theology but rebelled against efforts to limit his independence, developing his ideas of divine-right kingship in an intensely personal struggle to assert himself against the bullying to which Buchanan and others had subjected him.

Even more terrifying than Buchanan were the factious nobility of Scotland, who fought each other for control of James's person, forcing him to accept and cast off a series of ministers. Like Jenny Wormald, Lee argues that in learning to deal with the nobility, James developed into a tough and shrewd politician, adept at keeping his own counsel and maneuvering among rival factions ("James VI and I: Two Kings or One?" *History* 68 [1983]: 187–209). Unlike Wormald, however, he goes on to suggest that this Scottish apprenticeship was not an altogether ideal preparation for an English king who had to manage a more settled and formal state, requiring essentially different methods. James's desire to relax and enjoy himself once he had achieved his life's ambition by ascending to Elizabeth's throne did not help matters. Nor did the willingness of Robert Cecil and other English ministers to keep him relatively ignorant of English practices, so as to enhance their own power. Nevertheless, James scored some notable successes in England. He achieved more religious harmony than his predecessor partly because his Calvinist views more or less coincided with those of the majority of English Protestants. He also kept England out of costly European wars. But he failed to impose discipline on the English court—which Lee portrays, in traditional fashion, as riven by scandal and indecorum—and spent extravagantly on wasteful luxuries such as court masques. He thereby undermined the court's moral standing, although without fundamentally weakening royal authority.

Space does not permit adequate discussion of Lee's judgments of the many facets of James's English career. This is, however, an intelligent book that

deserves serious attention from all students of the period.

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KENNETH FINCHAM. *Prelate As Pastor: The Episcopate of James I*. New York: Clarendon Press of Oxford University Press. 1990. Pp. xvii, 360. \$82.00.

Revisionism concerning the reign of James I has been prominent in recent books. Both Maurice Lee in *Great Britain's Solomon* (1990) and Conrad Russell in *The Causes of the English Civil War* (1990) contrast James's relative success, for example, as "supreme governor," with the relative failure of Charles's reign, which produced sharp religious divisions.

Now Kenneth Fincham has produced an intensely focused study of the Jacobean bishops as a vehicle for his own revisionist contribution. He takes as his point of departure Hugh Trevor-Roper's assertion that the episcopate between 1610 and 1628 was a group of time-serving careerists who ignored their spiritual responsibilities, then proceeds to revise both this judgment and the time frame. He begins in 1603 with James's accession rather than in 1610 when Archbishop Richard Bancroft died and was succeeded by George Abbot. He documents Abbot's career as an active administrator and able preacher. Fincham sees continuity with the Elizabethan episcopate in the Calvinist theological orientation of most Jacobean bishops, including Abbot. But after James died there was a dramatic change. The author uses John Cosin's sermon at the consecration of Francis White in 1626 as bishop of Carlisle to signal the inauguration of the Caroline Arminian ascendancy.

Fincham sees in these two groups—Calvinist and Arminian—contrasting visions of churchmanship: the Pauline ideal of the preaching pastor and the Arminian ideal of liturgical uniformity, deemphasizing preaching, emphasizing instead worship as prayer and the efficacy of the sacraments. In their churchmanship, the majority of the Jacobean bishops represented the ideal of the preaching pastor, a minority the new Arminian ideal.

Fincham reinforces and amplifies what Lee and Russell have said: the Jacobean church under firm royal direction allowed for some diversity of opinion. James, however, made a clear distinction between "moderate" and "radical" puritans. The radical puritans he purged in 1604–05. James also insisted on subscription to the Thirty-nine Articles, the Prayer Book, and the royal supremacy. Moreover, in his "Directions on Preaching" (1622), he forbade discussion of contentious topics such as predestination. Consequently, for more than two decades the Church of England enjoyed relative internal peace.

In Charles's reign, when a different ideal of churchmanship prevailed, the conformity to that

standard was enforced; the church was divided against itself. Because of Charles's support for Arminianism, he was embroiled in controversy. This shift in churchmanship after 1625 became a vital element in the background to the Civil War.

The bulk of the book is devoted to establishing for the first time a comprehensive view of episcopal activity in James's reign. The basis for the author's judgments is extensive research in episcopal archives as the bibliography of source materials attests. The author documents in detail the bewildering variety of demands on a bishop's time. In national politics, increasing numbers of bishops were privy councillors (there were ten in 1623), and all bishops sat in the House of Lords. Fincham, however, attempts no estimate of their influence in these capacities.

Bishops had an important role to play in local society, performing many political and legal functions. Some were members of commissions of the peace in one or more counties. Most made, as required, periodic visitations of their diocese and participated in the functioning of local ecclesiastical courts. Bishops examined the fitness of men who presented themselves for ordination. In the south most candidates were university graduates, whereas in the north there was a dearth of well-educated men and of suitably endowed benefices.

Many episcopal functions presupposed that the bishop worked harmoniously with his chancellor and other diocesan officials who held life tenure, whereas such relationships were sometimes marked by acrimony. Because preaching was important to many Jacobean bishops, they delivered sermons in their dioceses as well as at court. Most were conscientious in licensing clergy to preach for their dioceses. Fincham examines each of these (and other) aspects of a bishop's responsibilities and shows who among the episcopal bench succeeded and who failed in performing such functions.

The author has succeeded admirably in transforming his dissertation from 1985 into an important first book, a book intensely but not narrowly focused. He is always careful to set the Jacobean evidence in a broader historical context. Fincham must now be considered an important young ecclesiastical historian.

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THOMAS COGSWELL. *The Blessed Revolution: English Politics and the Coming of War, 1621–1624*. (Cambridge Studies in Early Modern British History.) New York: Cambridge University Press. 1989. Pp. xii, 349. \$59.50.

Thomas Cogswell's book has a two-fold purpose: to examine the context of England's *renversement des alliances* in 1624 when a Spanish alliance was exchanged—briefly and disastrously—for a Franco-

Dutch one, and to test the Parliament of 1624 against Conrad Russell's thesis, which portrayed the early Stuart parliament as a declining institution employed chiefly as a cockpit for Court intrigues.

Cogswell begins with a vivid portrayal of England's relief at the return of its crown prince, the future Charles I, from an all-or-nothing embassy to the Spanish court in 1623 designed to force the issue of a marriage treaty and the recovery of his brother-in-law's patrimony, the Palatinate, from Habsburg clutches. Charles's empty-handed return was seen not as the diplomatic fiasco it was but as a national deliverance, for the Spanish match was desperately unpopular, and there was a good deal of at least rhetorical sentiment for a Protestant alliance to stem the tide of Counter Reformation on the Continent. Cogswell is persuasive in describing the nation's sense that it had escaped a peril as great as that posed by the invasion of the Spanish Armada in 1588 or the Gunpowder plot in 1605. He lodges an important point in positing this trinity of deliverances as part of the emerging mythology of England as a "redeemer nation."

A rupture with Spain required the calling of parliament, and Cogswell's middle chapters are taken up with the Parliament of 1624. As he notes, 1624 provides an ideal test case for Russell's paradigm, for in no other Stuart parliament did Court factions vie so openly as the war party strove to overcome King James I's remnant Spanish hopes and the influence of the still-powerful Hispanophile lobby. Charles and the Duke of Buckingham, the royal favorite, built up a "patriot" group among M.P.s and lords, many of whom had been jailed or exiled for opposition to Spain in the previous parliament. Cogswell's account suggests that this group was more fluid than either he or Russell describes it, as M.P.s such as Coke, Sandys, and Phelips, ambitious for place but no mere trimmers, deserted at crucial junctures to repair their standing with "country" backbenchers. Nonetheless, the thrust of Cogswell's analysis is clearly critical of Russell. It warrants attention, for Russell's thesis has colored recent accounts of the English revolution and, with them, central questions about the development of modern government, representative institutions, and the revolutionary tradition.

Cogswell's concluding section describes the post-parliamentary mood of the country as it prepared for war in the wake of the broken alliance. His judicious use of sources, printed and primary, makes clear the breadth of support for the war, a support that was to dissipate rapidly in the face of strategic indecision and military failure. One wishes that his account had pressed on to these events and given the book a firmer final outline. Nonetheless, it is an assured monographic debut by an able new historian of early Stuart England.

ROBERT ZALLER
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DONNA T. ANDREW. *Philanthropy and Police: London Charity in the Eighteenth Century*. Princeton: Princeton University Press. 1989. Pp. x, 229. \$32.50.

This excellent monograph is a study of the efforts of London philanthropists to put their money where their mouths were. Donna T. Andrew takes as her starting point eight large public charities: the Foundling Hospital, the Marine Society, the Lambeth Asylum, the Lying-in Charity, the Magdalen Hospital, the British Lying-in Hospital, the Philanthropic Society, and the Lock Hospital. She uses these examples to depict the relationship between charitable giving and broader ideas about the goals of philanthropy, the position of the poor, the nature of the economy, and the needs of the state.

Andrew states that, whereas medieval charity viewed indiscriminate giving as beneficial to the donor, modern charity stressed the need to choose recipients with care. Donors believed that properly directed charity could contribute to a national effort to reduce poverty and control delinquency. In the early part of the eighteenth century, writers stressed the relationship between a growing population and a booming economy: several charities of this period, such as the Foundling Hospital and the Lying-in Hospital, were intended simply to save lives.

Later in the century, donors became more exacting, requiring that institutions show that they used money sparingly and efficiently. Competing with each other for funds, the charities sought to offer donors value for money by putting inmates to work, cutting expenses, and choosing candidates carefully. For example, charities serving children preferred to take boys and older children because they cost less to maintain and their work was more profitable. Similarly, home-based charities, such as the Lying-in Charity, prospered at the expense of more costly residential maternity hospitals.

At the end of the century, the rationale of such charities came into question. Political economists denied that population growth was desirable in itself and philanthropists began to feel that the methods of the older charities were misconceived: instead of saving lives or providing employment the charities should improve public morality. Charity itself should not undermine the independence of the poor and so should be available only in occasional emergencies. Instead, philanthropists should promote schemes such as friendly societies and savings banks to help the poor help themselves. Donors were increasingly expected to contribute time and expertise in place of cash. At the same time, the goals of the charities changed from enhancing the wealth, military strength, and international prestige of England to creating harmony between the classes: it became evident that the enemy was within.

The book begins with clearly stated and important questions about the material, the case is carefully and persuasively argued, and the narrative is well fo-

cused. Andrew's mastery of the primary sources is especially impressive. She has uncovered much previously neglected material and has shown that it can support an original approach to an important topic. The overall impression is of an author who is determined to present her material clearly and fairly.

In the interests of balance, I would like to make three criticisms. First, the book lacks any reference to the "real" nature of the problem that confronted philanthropists. There is a wealth of evidence on this subject that cries out for integration into the study. Even very brief mentions of changes in the level of London mortality or wage rates would have been helpful.

Second, the book is more sensitive to the gender and status (or occupation) of donors than to their religious and political affiliations. Nonconformists often had a distinctive approach to both political economy and charity that disappears in this study: for example, the Unitarian John Aikin is quoted as an example of "evangelical" thought (p. 166).

Third, the design of the study creates a circular argument. The author has decided to concentrate on charities that sought to benefit the nation as a whole by improving the poor. She omits not only charities that cared for their own poor, such as the Huguenot charities, but also organizations such as the Thatched House Society that merely "relieved" the poor without trying to reform them. It is therefore inevitable that she would conclude that national concerns dictated the goals of philanthropy. A study that focuses on these "other" charities might interpret the dialogue about poverty and dependency differently, although Andrew's general picture should withstand further investigation. Nevertheless, anyone interested in eighteenth-century social history or the history of philanthropy would profit from reading this book.

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FRANK O'GORMAN. *Voters, Patrons, and Parties: The Unreformed Electoral System of Hanoverian England, 1734-1832*. New York: Clarendon Press of Oxford University Press. 1989. Pp. xiv, 445. \$78.00.

Frank O'Gorman's exhaustively researched and skillfully written monograph unquestionably takes its place as the leading study on the last century of the unreformed English and Welsh (but not Scottish or Irish) electorate. Following the pioneering but inevitably more limited works of W. A. Speck, John Cannon, and John Phillips on English constituencies and poll books, O'Gorman neatly redefines the scope of the English political experience during one of its most crucial periods. In doing so, he necessarily redirects the attention of the eighteenth- and early nineteenth-century British political historian away from the world of "high politics" (whether defined

from the perspective of Westminster or, as in the case of Lewis Namier, from a near obsession with aristocratic and patronal control over constituencies) to the voter.

Politics to O'Gorman represents a "teeming underworld" that "has never yet been prised open and penetrated" (p. 67), full of partisan musical bands, ribbon makers, banner carriers, electoral committees, election agents, returning officers, tally captains, and local clubmen. I have never read a more stirring or convincing account of the liturgy or rituals involved in the voting process, from the presentation of candidates to a description of the actual hustings to the final and tumultuous "chairing" of the winners. Descriptively and analytically, O'Gorman has rescued from clearly unwarranted neglect "the individual elector's great moment" (p. 136), the act of voting.

Through the use of quantitative analysis, the author means to achieve a modest rehabilitation of the pre-1832 electorate from those historians singularly more impressed with the political involvement of either Triennial England (1694–1716) or of the early and mid-Victorian age. In the process, familiar and established contours of British politics, the importance of 1832, or the existence of a deferential and venal, if not corrupt, electorate scarcely survive. For example, O'Gorman shows that between 1689 and 1832 the electorate as a percentage of adult males wavered between 14 and 25 percent depending on population growth and voter turnout. The change in 1832 involved, as best as O'Gorman can determine, the growth of an unreformed electorate of 14 percent of adult males to a reformed one of 18 percent, or, as he pithily remarks, "scarcely the stuff of which political revolutions are made" (p. 182). The reform bill of 1867, which entitled 54 percent of adult males to vote, was the key event in the development of a nascent democracy.

O'Gorman becomes moderately Taylorian in his view of the entire period of 1689–1866 as an age of continuity and relative stability. Thus, the crisis events of 1830–32, to discover the roots of which many historians have devoted their productive lives, were merely accidents occasioned by the untimely death of one king, the unexpected popularity of a new one, the disaffection of the Tory Right over Catholic emancipation, and the Swing riots, rather than occurrences necessitating deep-seated explanations based on the rise of radicalism or deference theories.

Perhaps surprisingly, the author, a distinguished historian of party development, is slightly less insightful in the section dealing with party than in the section dealing with voters. He purports to discuss how parties, through the medium of, for example, Fox or Pitt clubs or the partisan press, were the vehicles used to link together local and national issues. Yet, beyond a poorly defined notion of "independency," he never quite comes to grips with what these local parties that linked up with Westminster

stood for. There is virtually no discussion of Jacobinism or Jacobinism, of Catholics or Dissenters, or of Anglican loyalties or Anglican prejudices.

But these are minor criticisms. O'Gorman has successfully rescued the unreformed electorate from "the Whig interpretation of modern British history" (p. 4) just as his mentor, Herbert Butterfield, did in another context. All students of the period should be immensely grateful.

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PAUL DAVID NELSON. *William Tryon and the Course of Empire: A Life in British Imperial Service*. Chapel Hill: University of North Carolina Press. 1990. Pp. xiii, 250. \$24.95.

In 1765, William Tryon was appointed deputy to the governor of North Carolina. The following year he succeeded to the governorship. Tryon's personal papers have been lost; consequently this study is less a biography than, as Paul David Nelson's subtitle indicates, a study of Tryon's life in the British imperial service. There is little to indicate that Tryon was qualified to serve as royal administrator of North Carolina or any other colony. At the age of twenty-two he received a commission in the British army and had, by the close of the Seven Years' War twelve years later, achieved the rank of lieutenant colonel. His wealthy wife was, however, related to the president of the Board of Trade and Plantations. After six years in the southern colony he succeeded the Earl of Dunmore as governor of New York. Dunmore would have preferred that Tryon be sent to Virginia, pointing out to officials in London that his rival was "perfectly a Stranger" both to the Old Dominion and New York (p. 88). Tryon evidently knew little of North Carolina when he was sent to his first post in America.

Nelson sees three aspects of Tryon's career in imperial service as significant. First, his tenure as a faithful and, according to Nelson, talented supporter of the prerogatives of the Crown serves as a case study of Whitehall's ineptitude during the decade preceding the outbreak of the American revolution. The administration in London failed to heed Tryon's admonition that peaceful control of the colonies depended on abandoning claims of parliamentary taxation. Second, as a major general who led marauding raids against the Whigs, Tryon represents the views of the hardliners who dissented from the more conciliatory commanders, Sir William Howe and Sir Henry Clinton. And, last, Tryon's career is part of the historical literature of the loyalists, those who supported the lost cause of empire. Inasmuch as Nelson devotes less than two pages to this subject, his contribution in this respect is negligible.

Nelson's treatment of Tryon's administration in

North Carolina follows the traditional view of the struggle between royal governor and representative assembly. This is familiar imperial, Whig history, an old story set down by scholars more than a generation ago. In the sectional struggle known as the Regulator revolt, Tryon sided with the eastern magnates. Nelson does not go beyond the traditional explanation for the dissatisfaction of the Regulators over taxes, the court system, and representative apportionment. Nelson credits Tryon, once he had been transferred to New York, with effectively managing the enactment of laws appropriating funds for the support of British troops in the province. But this was hardly a difficult task, for the assembly had linked such monetary appropriations with a demand that the royal government allow the local legislature to issue paper money. A year before Tryon arrived in the colony the provincial agent in London had secured parliamentary permission for the colonial assembly to issue bills of credit. Tryon's task was therefore relatively simple. During his regime in New York, Tryon consistently favored the great landed families and land speculators. Once relations between the colonists and the royal government reached a crisis in 1774, the administration of Lord North did not spurn Tryon's suggestion to make concessions on taxation, as Nelson implies, but it was much too late, for, contrary to Tryon's assessment, influential Americans had gone far beyond this issue. Once hostilities broke out, Tryon reversed his position, advocating and carrying out harsh measures. Often at odds with the commander-in-chief of British forces, Tryon in 1780 finally secured permission to return to Britain. The picture that emerges from these pages is less that of an able, successful royal administrator than of a self-serving officer who did very well for himself financially. Although he lost much of the land that he had acquired in North Carolina and New York and had to spend part of his wife's estate of £30,000, he received more than £13,000 from the royal Treasury in compensation. In 1783, at the age of fifty-four, he retired with an annual income of £2,300, enough to provide "a sumptuous, elegant standard of living" (p. 177). Comparing Tryon with other royal executives in America during these years, Nelson ranks him second only to Thomas Hutchinson in terms of ability and accomplishments. As to Tryon's accomplishments, however, some may find them difficult to see.

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RODERICK FLOUD *et al.* *Height, Health, and History: Nutritional Status in the United Kingdom, 1750–1980.* (Cambridge Studies in Population, Economy, and Society in Past Time, number 9.) New York: Cambridge University Press. 1990. Pp. xxi, 354. \$59.50.

This is the most significant cliometric study to date in British social history and its exacting analysis of massive military anthropometric statistics casts new light on the controversy over the Victorian standard of living. It would be a pity, therefore, if its relentless statistical analysis frightens off historians. The book is replete with graphs and tables, and its pages are full of analysis of dummy variables, smooth curves, quantile bend estimation, reduced sample maximum likelihood estimation, bootstrap repetitions, unweighted curvilinear cubic regressions, bell-shaped curves, Kuznets curves, and weighted linear regression lines. But fortunately it is not necessary to be a statistician to grasp the reliability of the book's approach and the important conclusions that ineluctably emerge from it.

From their meticulous investigation of the many military and societal factors governing the supply of recruits, the authors conclude that British soldiers, notwithstanding the Duke of Wellington's epithet labeling them the "scum of the earth," were in fact anthropometrically indistinguishable from the working classes as a whole and that they "did not over the century constitute a particularly deprived group" (p. 10). Average height increased from the middle of the eighteenth century through the late 1820s, but then began to decline and continued to fall so that, even though a modest rise occurred for those born after the 1860s and who reached adulthood in the 1880s, it was not until the twentieth century that average male heights attained earlier levels. The authors allow themselves, in conclusion, a rare emotional sentence in an impeccably objective and dispassionate text: anthropometric trends indicate "that it was only by the end of the nineteenth century that improvements in real wages, and in public health and other sanitary measures, compensated the British working class for the horrors of urban and industrial life which they had borne in the second quarter of the century" (p. 319).

Statistics, said William Farr of the Registrar General's Office, formed "an arsenal for sanitary reformers"; consequently, right-wing historians are as suspicious of them as they are of supporting "impressionistic" evidence of debilitating ill-health and poverty persisting amid progress. They dismiss those historians who stress disease and debility as being morbid pessimists, captives of Charles Dickens's allegedly pernicious excremental visions and of their own overwrought sensibilities. This profoundly researched anthropometric study offers a stern corrective to the "optimistic" view of Victorian urban living standards and confirms what the "pessimists" (myself among them) have hitherto argued: low urban sanitary and housing standards continued in the late nineteenth century to hinder England's progress toward being a fitter nation. Indeed, this book is an outstanding example of how cliometrics tends to reinforce rather than reinterpret more traditional social history. Victorians, using their eyes and con-

ducting massive social surveys, had it right: real wages were rising, general death rates were declining, and diets were improving, but the urban environment had, partly through its detrimental impact on infants at crucial developmental stages, created a new phenomenon (one that was especially frightening in its Darwinian implications of degeneration), the survival of the "sickly urban type." This scholarly work provides substantial anthropometric proof for the Medical Board's devastating conclusion in 1919: "of every nine men of military age in Great Britain . . . three were perfectly fit and healthy; two were . . . definitely infirm . . . three were incapable of undergoing more than a very moderate degree of physical exertion and could almost . . . be described with justice as physical wrecks; and the remaining man was a chronic invalid with a precarious hold upon life" (p. 63).

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ROBERT HOLE. *Pulpits, Politics, and Public Order in England, 1760–1832*. New York: Cambridge University Press. 1989. Pp. xiv, 326. \$54.50.

Recent historical literature on the religion and politics of late eighteenth- and early nineteenth-century England, from the writings of Jonathan Clark to those of Boyd Hilton and J. A. Parry, have emphasized the persistent centrality of religious sensibility to the conduct of politics. Robert Hole's well-conceived and elegantly executed book, although intended as a contribution to this new scholarly emphasis, actually dissents from it in an important way. Hole aims to examine the "use of religious arguments in political and social theory" (p. 5) and concludes that from the 1790s secular arguments gradually supplanted purely religious ones in clerical (establishment) discourse. This secularization of clerical rhetoric occurred in response to the French revolution and socioeconomic conditions in post-1815 England; the church managed the rhetorical shift internally and owed nothing to pressures emanating from deists and radicals.

Hole divides his study into three parts, each conforming to a different phase of the political revolutionary process: the "pre-revolution," 1760–89; the "revolution proper" until 1804; and the "post-revolution" era, 1804–32. Each part contains a mix of chapters intended to establish the prevailing character of political, philosophical, and social theory; the social context of these ideas; and a case study of a religious figure whose activities illustrate the foregoing in detail.

The case studies are especially illuminating. During the first phase, an era dominated by older notions of political obligation and strictly philosophical reflection, an intellectual, William Paley, grafted a Utilitarian ethos onto the body of Christian thought. Samuel Horsley, a typical member of the establishment, during the second phase effected a synthesis embodying

the transition from political/philosophical to social theory. William Hone, a Unitarian and a reforming publisher, and by definition already something of a secularist, illustrated during the third phase the obverse of Paley and Horsley; although establishment thought and rhetoric grew more secular, radicals such as Hone frequently framed their political arguments in religious terms. The "infidelity and sedition" crowd had not remained, in other words, the only representatives of the reforming left.

The attention Hole gives to Dissenters (especially Unitarians), nonbelievers, and Catholic thinkers, while excluding the much-studied evangelicals, is refreshing and therefore welcomed. The coherent structure of his book, with its emphasis on evolutionary development, the clarity, consistency, and absorbing quality of his argument, and his sustained effort to contextualize as well as personalize the discussion of theory makes this work a model study in intellectual history.

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GREGORY CLAEYS. *Citizens and Saints: Politics and Anti-Politics in Early British Socialism*. New York: Cambridge University Press. 1989. Pp. xvii, 360. \$54.50.

As the title of this excellent study suggests, Gregory Claeys deals with a fundamental tension in British social thought during the first half of the nineteenth century. Out of the complex interplay of radical republican and millennial traditions reembodying in Chartism and Owenism, a new "social radicalism" emerged by mid-century. The author focuses mainly on Owenism to show the development of a new "language of politics," one that enabled social reformers to identify problems and possibilities not previously recognized.

This book presents an intricate argument based on a thorough command of the relevant secondary materials as well as impressive research into the sources: periodicals, pamphlets, and memoirs. Claeys's approach to political thought has been strongly influenced by the methodologies developed by John Dunn, John G. A. Pocock, and Quentin Skinner and their efforts to clarify the intentions of authors and analyze language to get at fundamental assumptions as well as contexts.

After discussing the traditions on which Owenism draws—not only republican but the conceptions of natural law or natural society and the millennial impulse associated with radical Puritanism—Claeys examines the development of Owenism. He pays special attention to the tensions within the movement between paternalism and democracy and between centralization and decentralization. In his discussion of the central goal of the Owenites, the creation of a new kind of community based on a "new social

science," the author argues that Owen and his followers remained faithful to the earlier republican commitment to personal liberty. And Claeys goes to some length to acquit Owenism of the charge of "proto-totalitarianism" that J. L. Talmon directed against the early forms of socialism on the Continent.

It was, of course, through an emphasis on the workings of the new economic system that the Owenites challenged traditional republican and radical thought. Claeys describes the way in which the Owenites employed the classical theory of political economy to develop a "fully-fledged alternative economic system" (p. 143). But, during the 1830s, the socialism of Owen was also politicized through a complicated interaction with the Chartists. Discussions of "Chartist complaints" about Owenism and "Owenite grievances" toward the Chartists clarify the obstacles to cooperation between the two movements. Claeys's account of their interaction is cautious; he recognizes the great diversity within each of the camps and the absence, for the Owenites, of the local studies needed to provide a fuller picture.

Not until the late 1940s, following the collapse of Owen's Queenwood colony and the defeats suffered by the Chartists, was the way open for a convergence of the two approaches to reform. By mid-century there was, however, a "broadly diffused . . . new social language . . . a paradigmatic reconceptualization with radical political thought, a new . . . consciousness of the inappropriateness of an older language . . . to the social, political, and economic conditions of mid-century. The Machiavellian movement had now largely passed. The Marxist movement was dawning" (p. 287).

Claeys has produced a rich and subtle treatment of Owenism and its contribution to British social thought. But the study also connects that thought with developments on the Continent. He notes, in particular, the impact of the revolutionary upheavals at mid-century on the British reformers. And through a discussion of the debt of the young Friedrich Engels to Owenism, Claeys shows connections with Marxism. Indeed, one of the many merits of the study is to place Owenism more clearly within the larger development of both European and British socialist thought.

STANLEY PIERSON
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R. J. MORRIS. *Class, Sect, and Party: The Making of the British Middle Class; Leeds, 1820-1850*. New York: Manchester University Press; distributed by St. Martin's, New York. 1990. Pp. xi, 356. \$59.95.

The title of R. J. Morris's long-awaited study reveals much about the character and intent of his work. The subtitle, one suspects, reflects his original purpose, formulated in the late 1960s, of using Leeds as a social history laboratory within which the making of

the nineteenth-century British middle class could be traced. Two decades later, after much research, rethinking, and what is probably the most ambitious computer-based record linkage project ever undertaken in the field, Morris has concluded, as the main title indicates, that religious sect and political party constituted more fundamental sources of middle-class identity and that they operated in ways that distorted and, to some extent, preempted an overarching bourgeois consciousness of class.

To be sure, Morris is well aware that class bifurcation was structured into the logic of early nineteenth-century Leeds' social development as the textile industry expanded and as industrial capitalism spread, albeit unevenly. Nevertheless, he emphasizes the fluidity and diversity of the urban industrial labor force, the persistence of socially mixed residential neighborhoods, and the artificiality of dualistic, "two nations" interpretations that recognize only the extremes of rich and poor. Popular culture and working-class consciousness were, it follows, correspondingly fragmentary and complex. Class relations in Leeds ranged alternatively between "conflict, consensus, deference, and defiance" (p. 119), depending on the particular groups and circumstances involved. Far more significant than class in the formation of middle-class identity were, he argues, distinctions of status and occupation, as well as cultural and political divisions between Whig/Nonconformists and a more established Anglican/Tory elite. Middle-class unity was, at best, partial and intermittent, exhibited primarily within voluntary associations devoted to specific "class projects" such as the Philosophical and Literary Society or subscription relief funds for the poor. Even here, class-wide unity proved to be precarious, depending on the active leadership of wealthy commercial and professional patrons and requiring strict adherence to a "no religion, no politics" rule.

As those familiar with his earlier work will have anticipated, Morris makes an important contribution to a burgeoning field. His work is meticulous, and his judgments are discriminating and massively grounded in empirical research. Indeed, Morris is at his best in his handling of specific evidence, particularly in his astute and creative use of recalcitrant but potentially quantifiable data from directories, poli-books, and local organizational records to construct a political, religious, and economic profile of the Leeds middle class. Nevertheless, the care and caution with which Morris handles his chosen materials is not achieved without some general explanatory cost. He is absolutely right to highlight the complex, intraclass distinctions and antagonisms that fractured the middle class from within. His larger retreat from the intellectual framework of class analysis, however, can be made to appear plausible only by deliberately downplaying the most explosive and dramatic social movements and developments of his period. Chartist, Owenite socialism, opposition to the Poor Law,

and factory reform scarcely appear on Morris's pages, although they were omnipresent in Leeds during the 1830s and 1840s. While the class conflicts between capital and labor that they embodied may have been less destabilizing in Leeds than in some other places, they remained inescapable realities that confronted middle-class people whether they liked it or not.

In fact, Morris has passed up a golden opportunity to demonstrate how the Leeds middle class's internal political and religious divisions represented two alternative and, to a degree, mutually incompatible approaches to resolving the class antagonisms that early industrial capitalism generated. By striving to incorporate workers into a new consensual order that was framed alternatively in conservative paternalist or in liberal market terms, different middle-class groups contended with one another to establish the framework within which their larger class hegemony would be exercised. Although neither group could triumph unconditionally, it was the very struggle between them during the 1830s and 1840s that laid the foundations for the consensus that was actually achieved during the mid-Victorian period by pragmatically fusing key elements from both. Precisely because sect and party were so important within the Victorian city, it is all the more essential that we understand the ways in which they actively reinforced rather than merely attenuated the making of the British middle class.

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DAVID J. V. JONES. *Rebecca's Children: A Study of Rural Society, Crime, and Protest*. New York: Clarendon Press of Oxford University Press. 1989. Pp. ix, 423. \$72.00.

David J. V. Jones's book has appeared just in time to help mark the 150th anniversary of the Rebecca disturbances of the early 1840s in southwestern Wales. Previous generations have by no means ignored these protests, but it is only with the publication of this study that the many myths and misunderstandings surrounding Rebecca have been carefully dissected and, in the main, put to rest. No longer tenable is an older view that regarded the riots as the work of small farmers who, driven by economic desperation, attacked and destroyed the tollgates of profiteering turnpike trusts. There remains, of course, a good measure of truth in this interpretation, but Jones paints a far more variegated and vivid picture. In addition to being more violent, widespread, and prolonged than has been supposed, the disturbances expressed deeply held communal values, encompassed broad segments of local society (albeit not gentlemen), entailed more than assaults on tollgates, and were remarkably organized and disciplined affairs.

In some of these aspects, Rebecca will not appear

unique to those who are familiar with recent literature on tumults in other parts of Britain in this and earlier periods. It is not surprising, then, that the issue of causation should compel Jones to examine more general transformations in nineteenth-century rural and urban life in addition to those that were distinctly Welsh in character. There is no question that Rebecca arose in circumstances that were quite specific to Wales, indeed, southwestern Wales. Unusual settlement patterns, an equally unusual structure of landownership, the existence of ethnic and linguistic animosities, and the growing importance of rural Dissenting religion had identifiable influences on the origins and the nature of the disturbances. Yet Jones's analysis gives pride of place, at least in terms of sheer bulk, to the usual, depressing story of an early nineteenth-century British rural population coming under an avalanche of mounting pressures: population growth, the extension of market relations in conditions unfavorable to wage earners and marginal producers, the collapse of customary usages and perquisites, the oppressions of the New Poor Law, and the increased burdens of exploitation in the forms of higher rents, rates, tithes, and tolls.

This perspective gives Jones's book, on the one hand, considerable richness and wider significance as a comprehensive study of social and economic conditions in Wales in the first years of Victoria's reign. On the other hand, this very virtue can and does lead to some difficulties of interpretation. Clearly, a phenomenon as complex as Rebecca did not have a single cause or cluster of causes. The challenge, therefore, is to be broad in approach but discriminating in focus so as to avoid simply cataloging an array of causal factors without regard for their relative weights of importance and the linkages between them. If Jones on occasion appears to fall a bit short in meeting this challenge, it very well may be because he insists on defining the Rebecca phenomenon in the broadest terms possible. This book is not merely about riots led by men on horseback dressed as women. Its discussion of "Rebecca's children" and "Rebeccaism" embraces the greater part of an entire society and a universe of social action, from the use of "rough music" in the enforcement of community standards to arson and the individual opportunism often found in general mayhem.

This may trouble those readers who are equally insistent on the need to define protest, especially violent protest, with some precision and to locate causes (which in turn helps to define protest) in fairly specific grievances or problems. Nevertheless, even those of this disposition will no doubt find much in this book to be worthy of close attention. The issues discussed often transcend the peculiarities of the Welsh and their experiences, while at the same time the reader will be treated to what must be regarded as the best account of both Rebecca's children and

Rebecca herself, 150 years old and still a fascinating lady.

PETER DUNKLEY
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JEFFREY G. WILLIAMSON. *Coping with City Growth during the British Industrial Revolution*. New York: Cambridge University Press. 1990. Pp. xxi, 344. \$54.50.

This work adds to Jeffrey G. Williamson's distinguished contributions to the study of both economic history and contemporary economic development. Williamson's book sets out to accomplish several objectives: to test some of the most broadly accepted hypotheses and assumptions about British urban growth in the era of early industrialism; to use analyses of modern Third World urban growth to develop new perspectives on the British urban transition; occasionally to work the problems in reverse and use the British experience as a basis from which to assess modern economic processes in the Third World. For the most part, it succeeds splendidly, developing a brisk and stimulating review and revision of common ideas about both past and present conditions and processes.

Using the analytical tools of a Third World economist and reinterpreting the British data from new perspectives, Williamson advances many persuasive arguments. For example, after a thorough examination of Irish immigration, labor absorption, and wage rates, he concludes that Irish immigration neither lowered the living standards of the laboring classes nor fostered industrialization. He does, however, argue that they slowed rural out-migration and that "Had there been no Irish in Britain, agriculture would have suffered far more than industry" (p. 154). Analyzing the census data from 1851 by using the methods of a modern economist, Williamson advances clear answers to long-discussed questions about, for example, the ages and earning capacities of migrant and nonmigrant urban workers. He argues that "Britain's cities absorbed the flood of migrants with considerable ease" (p. 123). Indeed, his general conclusion seems to be that, given the perspective of the Third World experience with the urban transition, the process in Britain was neither as unusual nor as dreadful as most traditional interpretations have painted it.

The quality and detail of both evidence and process that characterize most of this book make its arguments persuasive and demonstrate the value of economic analysis and comparative approaches. The last chapter, however, also illustrates the hazards of pushing a good thing too far. For example, in arguing that "Britain" exercised a "choice" in the underinvestment in its early industrial cities, he maintains that Edwin Chadwick's Report of 1842 had been "preceded by at least half a century of accumulated understanding and public health experimentation" (p. 282) without

providing evidence or discussion of what this was, where, by whom, and what practical applications were generated. In this chapter, the weaknesses of a narrow economic vision of the operation of a nation are also evident. No society, modern or otherwise, makes rational policy decisions entirely on the basis of detailed analyses of quantifiable economic operations—social and cultural expectations as well as political realities also play major, sometimes dominant, roles. Williamson argues that the social reformers were well aware of the cost-benefit analysis of urban sanitary reform and implies that national leaders made a conscious choice to underinvest in "city social overhead." Modern non-Western governments have access to a far wider range of policy models than were available in early nineteenth-century Britain and they work within very different frameworks of assumptions about, and expectations of, governmental action.

The generation of British leaders that experienced the French revolution and fought the French wars became so averse to new initiatives of any kind that the modest reform agenda initiated in Britain in the later eighteenth century was put on hold for several decades. Considering personal self-interest, degree of economic sophistication, and social conscience, it is hard to see how the same political leaders who produced the Corn Laws could have also developed farsighted policies to encourage investment in urban social overhead, or how those who passed the Six Acts (1819) could have been guided by a cost-benefit analysis of the health of the urban working class.

Although the author outruns his evidence in the concluding chapter, his book's value and stature are unquestioned. He presents old questions in new ways, offers many interesting and innovative new answers, and provides an important work for both British historians and economists of the contemporary Third World. Scholars working on nineteenth-century British cities, as well as on such topics as public health and labor history, will be both informed and challenged by Williamson's study.

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ELLEN GIBSON WILSON. *Thomas Clarkson: A Biography*. New York: St. Martin's. 1990. Pp. xiv, 269. \$39.95.

Although anyone familiar with the story of British antislavery will know the name of Thomas Clarkson, his place as "the originator" of the movement is generally overlooked. Pride of place is accorded to William Wilberforce, with Clarkson dubbed an organizer, salaried field worker, or some comparably subordinate designation. Such was not the case during Clarkson's lifetime or immediately thereafter. His fall from renown, Ellen Gibson Wilson argues, was the result of the uncharitable treatment accorded him in the five-volume *Life of William Wilberforce* (1838),

written by Wilberforce's sons, Robert and Samuel. They retaliated for Clarkson's supposed slighting of Wilberforce and the elder James Stephen in Clarkson's *History of the . . . Abolition of the Slave Trade* (1808). Clarkson's work concentrated on the extra-parliamentary agitation and said little of the then younger generation of antislavery men with whom Stephen was associated.

Wilson follows Clarkson's journey from a brilliant undergraduate career in St. John's College, Cambridge (crowned by winning the senior bachelors competition on the question of the legality of slavery) to the world of Quaker activists into which he threw himself. His organization of the nonsectarian Abolition Committee and his tireless investigation of the trade in English ports provided the bulk of evidence brought before the parliamentary and privy council investigative committees set up in response to the initial parliamentary effort. Clarkson's sympathy with the French revolution distinguished him among the abolitionists and, along with his Whiggish love of liberty, embarrassed Wilberforce and those leading the parliamentary struggle. Exhaustion and the setbacks caused by the revolution prompted a decade of retirement, during which time he married, led the life of a gentleman farmer, and established lifelong friendships with William Wordsworth, Dorothy Wordsworth, and Samuel Taylor Coleridge. Resuming extraparlimentary activity in 1804, he devoted fifteen years following the abolition of the trade to occasionally indiscreet diplomatic efforts on the Continent designed to persuade leaders of other European states to follow Britain's example. Clarkson resumed vigorous domestic agitation with the commencement of the campaign for emancipation in 1822 and, except for the contretemps with Wilberforce's sons, received increasing recognition and acclaim from the Emancipation Act of 1833 until his death in 1846.

A less effective attempt to restore Clarkson's reputation was made some fifty years ago in Earl Leslie Griggs's old-fashioned study, *Thomas Clarkson: The Friend of Slaves* (1936). Wilson brings to her admirable biography a greater array of manuscript sources, some uncovered by her in earlier works, as well as familiarity with scholarship of the last half-century. She deploys ample evidence and persuasive argument to demonstrate that Clarkson's indefatigable efforts on behalf of abolition not only preceded those of Wilberforce but made possible the parliamentary triumph with which Wilberforce is associated. Indeed, it was in Clarkson's mobilization of national moral indignation that he inaugurated an auspicious and impressive method of extraparlimentary agitation that was to be a model to emulate in subsequent causes.

Wilson has provided a welcome reassessment of a neglected and significant figure in a study written with clarity, precision, and occasional literary grace. It also reminds students of British antislavery that the

cause was hardly the exclusive domain of the Evangelicals.

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CHRISTOPHER PARKER. *The English Historical Tradition since 1850*. Edinburgh: John Donald. 1990. Pp. vi, 252. £20.00.

Christopher Parker studies texts, written largely by historians, that reveal the development of a historical tradition in England from 1850 to the 1970s. Beginning with the Liberal Anglicans and concluding in a post-World War II conservative cooption of earlier liberal values, Parker addresses the lack of reflection within mainstream historical writing. His aim is to persuade historians to study and confront their own discipline and its past. The historian's public duty is to understand the nature and focus of history, with its attendant problems of moral judgment, individual responsibility, and the possibility of predicting historical directions.

Beginning with Duncan Forbes's *Liberal Anglican Idea of History* (1952), Parker extends Forbes's emphasis on an empirical, liberal, individualist, autonomous historical method, emphasizing free will and moral responsibility within a progressive and providentially directed nation, to an enduring tradition of historical writing in Britain. He argues that individualism, set within national traditions and perceptions by English historians such as William Stubbs, Goldwin Smith, J. A. Froude, Lord Acton, Charles Kingsley, E. Lecky, J. R. Green, and Edward Freeman after the 1850s and 1860s, repudiated positivism by understanding history as the story of personal freedom and divine judgment and as a practical guide to social conduct and national cohesion. Then, T. H. Green, F. H. Bradley, and Bernard Bosanquet shaped the economic history of Arnold Toynbee, William Cunningham, W. J. Ashley, and George Unwin by making empirical methods compatible with idealism, the dominant intellectual influence in England after the 1870s, and by a largely organic solution of the conflict between the individual and society. Bradley's essay of 1874 on critical history appears as an anticipation of R. G. Collingwood's and Michael Oakeshott's dismissal of predictive science in favor of the uniqueness of history. But Parker contends that instead of following Bradley, historians confused science with the objective establishment of facts in archives to adopt a naive idealism that prevented them from understanding social causation. Parker finds that after World War I, although dominant Whig constitutional history was threatened by a politically conservative or "realist" administrative history and by an interest in international history that supplanted national interests, liberal individualism was revived and firmly established again by the 1930s. Individuality, opposed by communitarian values in 1914 and again in

the 1920s and 1930s, reemerges in the 1950s and 1960s in a new, conservative historiography typified by Lewis Namier. A reaction against scientific history and Idealism, both identified with Germany, made the individual the historian's proper subject. In spite of Collingwood's sense of isolation, Parker sees his philosophy of history as typical of the attitudes among historians from the 1930s to the 1950s, and especially of Maurice Powicke, Norman Sykes, W. K. Hancock, Eric John, and R. W. Harris. Parker admires the social and economic historians at the London School of Economics such as R. H. Tawney and Eileen Power, because, unlike the cautious mainstream historians of the interwar years, they provided intelligent and innovative methods. Marxist alternatives to newer forms of liberal and conservative historiography failed, despite a new humanism that borrowed Whig and Liberal Anglican approaches to history and a tradition that tried seriously to consider how history and theory were related. In conclusion, Parker identifies an ascendant conservative history, a liberalism without liberals, in the writing of Herbert Butterfield, Maurice Cowling, Geoffrey Elton, Oakeshott, and George Kitson Clark.

Parker has written a provocative and thoughtful book. But his reasons for emphasizing some writers such as, for example, F. H. Bradley, whose influence on historical writing is not demonstrated, while omitting others such as J. L. Hammond and Barbara Hammond, whose views on the nature of the individual within society became the center of a major historical controversy, are not clear. It would have been a still better book if Parker's texts were set more within specific historical time and place to reveal the relation between them and the intellectual, political, social, economic, religious, and idiosyncratic issues that they simultaneously reflected and fashioned.

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SHELLEY PENNINGTON and BELINDA WESTOVER. *A Hidden Workforce: Homeworkers in England, 1850–1985*. (Women in Society.) London: Macmillan Education; distributed by New York University Press, New York. 1989. Pp. xi, 191.

Women who work at home have usually done so to accommodate the demands and social expectations surrounding their roles as wives and mothers. And they have suffered for it. Homework, though it has taken many different forms, ranging from nail making to the assembly of matchboxes, has always been one of the most exploited forms of labor and one dominated by unskilled women. The advantages of homework are that it is readily combined with domestic responsibilities, usually requires little skill, has flexible hours, is compatible with child care, and, often most important, can be hidden from prying

eyes. Such work can obscure the deficiency of a male breadwinner's earnings and leave intact the appearance of traditional patriarchal domesticity. For various reasons, women have been prepared to accept sweated wages, social isolation, and unregulated working conditions long after legislation and inspection have improved the lot of other laboring people.

Shelley Pennington and Belinda Westover argue that "homework has not only exploited [women] but has also, to some extent, played a part in perpetuating women's subordinate socio-economic position." Conventions of respectability concerning appropriate women's work, the absence of alternative employment for married women, the lack of skills training, as well as poor public provision for widows and orphans and low and irregular wages for unskilled men all conspired to create a captive female labor force and branded such women "as subordinate creatures inferior to and dependent on men."

Drawing on a range of parliamentary papers and social reportage long familiar to labor historians, the authors present a survey of homework in England from 1850 to 1985. Their book surveys the types and geographic locations of various forms of homework, its role in the family economy, and its remarkable resistance to the efforts of legislators, social reformers, and trade unionists to bring about any significant and lasting reform. Despite many prominent studies of sweated labor in the late nineteenth and early twentieth centuries, little real headway was made in helping the lot of women homeworkers. More recently, since World War II, no significant legislation has been passed affecting the situation of the homeworker. The difficulties of organizing a dispersed and often economically desperate work force remain much as they were in the nineteenth century, compounded, perhaps, by the difficulties of reaching the large and more recent influx of vulnerable immigrant women in the postwar homework labor force. This volume serves as a sharp reminder that the dramatic improvements in most women's working experiences and domestic circumstances over the past century can all too easily obscure the persistence for other women of poverty-bound and socially constricted lives.

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CAROL DYHOUSE. *Feminism and the Family in England, 1880–1939*. New York: Basil Blackwell. 1989. Pp. vi, 204. Cloth \$37.95, paper \$14.95.

Over the past twenty years, historians have developed a sophisticated analysis of the emergence of new family forms and of a new ideology of domesticity in the nineteenth century. But, as Carol Dyhouse points out, less work has been done on the succeeding period. In her book, Dyhouse's intention is to offer a unifying overview of the relationship between femi-

nism and the family from the late Victorian years to World War II.

After an introductory chapter in which she outlines the contours of her subject, Dyhouse deals in three subsequent chapters with the issues of women's economic independence, the organization of domestic life, and marital relationships. In all four chapters Dyhouse analyzes a wide range of public discussion about these issues and intersperses that analysis with accounts of individual experience.

Dyhouse does indeed provide a helpful overview of middle- and upper-class feminist discourse on the family during this period. Like the author's previous work, this book is based on sound scholarship and it is clearly written. Dyhouse's insights illuminate the connections between seemingly disparate groups of women activists and writers. In the chapter on economic independence, for example, she integrates the work of the Fabian Women's Group in the period after 1908 with the historical scholarship produced during this generation by women such as Alice Clark.

But, while the book offers a lucid account of the issues with which it is concerned, its clarity is at times deceptive. Dyhouse—wisely, I think—avoids direct confrontation with much current poststructuralist theorizing about feminism and sexuality, but I do wish she had provided her readers with a more developed definition of both feminism and the family. At times she offers conjectures and conclusions that do not withstand close scrutiny. One instance is the opposition Dyhouse posits between Alice Clark's *Working Life of Women in the Seventeenth Century* (1919) and Ivy Pinchbeck's *Women Workers and the Industrial Revolution, 1750–1850* (1930), in which Clark is presented as “feminist” whereas Pinchbeck is characterized as “conservative.” What this dichotomy fails to address is the conservative nature of Clark's feminism (and, one might add, of the Fabian Women's Group). An even more important weakness is Dyhouse's failure to notice the issues such as wife beating and child sexual abuse, on which feminist discourse was largely silent during these years.

Although this book does have weaknesses, its strengths—its clarity and a sensitive treatment of its material—make it admirably suitable for students and general readers. And, for scholars in the field, this synthesis raises a number of interesting questions. As Dyhouse says in her introduction, her intention was to write “an exploration rather than a definitive account” (p. 6), and she has indeed achieved this important objective.

DEBORAH GORHAM
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HARRY HENDRICK. *Images of Youth: Age, Class, and the Male Youth Problem, 1880–1920*. New York: Clarendon Press of Oxford University Press. 1990. Pp. x, 298.

Until the 1970s, historians rarely allowed youth to play more than a peripheral historical role. The student protests of the late 1960s changed all of that, prompting historians John Gillis and Joseph Kett to go in search of the origins of the modern traditions of youth. Daring, long-range flights characterized this early work. But flying soon became hazardous, as the low cloud of the densely researched monograph fell. This study belongs to the second historiographical phase. In line with recent work, Harry Hendrick concentrates on the institutionalization of adolescents; his primary interest is with how, why, and with what effect the professional middle class perceived, defined, and sought to train the young male worker. His study breaks new ground, however, in its laudable determination to link the debate on “boy labor” with the political, economic, and racial anxieties of Edwardian Britain.

Adolescents constituted close to 16 percent of the Edwardian male labor force. A declining proportion were apprentices or “learners,” others were semi-skilled machine minders, and large numbers entered the commercial and distributive spheres of the city as messengers, van boys, and errand boys: like postcards, easy to send and cheap. From 1904, social investigators began to condemn the “blind alley” nature of the “fetching and carrying” occupations; the next generation, it was feared, would have little skill and less work discipline. For Hendrick, the significance of the debate on the “boy labor” problem (informed by the wider discussion on Britain's economic and military efficiency) is that it fused with the new social psychology of adolescence and with an older critique of working-class culture and character to reveal an image of adolescence as a distinct stage of the life cycle. In contrast to Gillis, who argued that the “discovery” of adolescence was a middle-class occurrence ushered in by the late-Victorian public schools, Hendrick insists on seeing it as an outcome of Edwardian anxieties concerning working-class youth.

The book succeeds admirably in tracing the interconnections between urban working-class youth and contemporary debates on poverty, unemployment, and casual labor. It presents the fullest and subtlest investigation of the boy labor problem, and it convincingly amends the existing account of the discovery of adolescence. These are considerable achievements. The book is based on extensive research and sound scholarship, although the quotation from William Beveridge (p. 61) makes little sense because of the unacknowledged omission of “casual” before “employment.”

The book, however, is not without its flaws. The decision to omit “girl labor” from the study was an unfortunate one. The anxieties surrounding female adolescents may have been different and the reach and strength of those anxieties less, but something on the images of the young female worker would have added the valuable dimension of gender. The final

three chapters on the solutions to the boy labor problem are also less than convincing.

A moratorium on the history of Edwardian youth groups should be declared; chapter 6 adds little or nothing to the subject. This part of the book would have been more effective, too, if instead of arguing for the significance of the juvenile labor exchanges and the compulsory day continuation schools, Hendrick had posed the question: why, given the linkage between boy labor and Edwardian fears about national decline, were the collectivist solutions to the evident problems of the juvenile labor market so manifestly ineffective? Only 8 percent of all school leavers in 1913 found their first full-time job through the exchanges; many of them still fetched up in blind-alley jobs. The handful of day continuation schools, which opened in response to the Education Act of 1918, all withered on the vine. Having shown that new images of working-class youth were forged in these years, that a new set of age relations came into being, Hendrick needs to say why the mechanisms for creating "adaptable" wage earners and responsible "citizens," considered essential for industrial and political harmony, failed to emerge.

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EMMET LARKIN. *The Roman Catholic Church and the Home Rule Movement in Ireland, 1870–1874*. Chapel Hill: University of North Carolina Press. 1990. Pp. xxi, 416. \$59.95.

Like Machiavelli's prince, Emmet Larkin is still engaged in his exercise in scholarly virtue: he has an abstract scheme of historical interpretation and ruthlessly is finding the evidence to bring it to fulfillment. This book, dealing with ecclesiastical affairs in Ireland between 1870 and 1874, is the sixth in a projected twelve-volume history of the Roman Catholic hierarchy and its political activities between 1780 and 1918. As in the previous volumes the work is based almost completely on the correspondence of the Irish prelates, particularly that of Paul Cardinal Cullen, which is found in the archives of the Irish College in Rome, that of Propaganda Fidei, and in the Dublin archdiocese collection. Within the strict limits that Larkin has imposed on himself the study is worthwhile, presenting the prelatical infighting of the time in a more thorough manner than that provided by Peadar MacSuihbne in his five volumes of Cullen letters. It certainly provides insights about the bishops' problems with Home Rule that supplement earlier studies, such as Edward R. Norman's *The Catholic Church and Ireland in the Age of Rebellion, 1859–1873* (1965).

The weakness of the work reflects Larkin's methodology. In a neo-Rankean fashion his "scientific" study of episcopal correspondence is intended to present an authoritative account of what was taking

place among the leaders of the Irish Roman Catholic Church between 1870 and 1874. This poses a hermeneutical problem, however, and not every historian would agree with Larkin's interpretation of his documentary evidence. As in the other volumes where he has used his "mosaic" technique, he is intent on showing how the Irish Roman Catholic Church was slowly identifying itself with the cause of Irish nationalism. Eventually, in Larkin's scenario, the two authorities were to find fulfillment in the Constitution of 1937. This viewpoint may have become a pillar of the American-Irish Roman Catholic school of history, but it is only one way of interpreting the evidence that Larkin so diligently presents in volume after volume.

Larkin's study begins with a consideration of the role of the Irish bishops in Vatican Council I, and he concludes that with regard to infallibility, Cullen took "a moderate line" (p. 4). I suspect that if he had moved beyond his beloved correspondence and consulted works like August Hasler's *Pius IX* (1977) or Christoph Weber's *Kardinale und Pralaten* (1978) he might have come to a different conclusion. Neither the Prussian nor the Bavarian ambassadors considered Cullen as any kind of a moderate. After all, Cullen, with Cardinal Luigi Bilio, did produce the formula on papal infallibility that the council finally adopted. As for the idea that "the Irish bishops as a body were able to contain Cullen" (p. 9), Hasler opined that the Irish bishops, most of them Roman educated, readily took advice from Propaganda and Cullen. None of them dared risk losing Roman financial support.

Similarly, Larkin's focus denies him an understanding of the O'Keeffe imbroglio, which brought up the issue of *praemunire* when the Kulturkampf raged in Germany. He also has no real explanation for Cullen's great personal dislike of Isaac Butt, for Cullen would be unlikely to comment on the moral antics of his next-door neighbor on Eccles Street in the 1868 period. Presentation of the hierarchy's correspondence is, of course, very valuable, but in the end it gives us a rather one-dimensional work. But then that quality may attend all mosaics.

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ANDREW GAILEY. *Ireland and the Death of Kindness: The Experience of Constructive Unionism, 1890–1905*. (Studies in Irish History.) Cork: Cork University Press. 1987. Pp. xiv, 345. £23.00.

A. B. Cooke and John Vincent's anatomy of "West-ministerial" politics during the pivotal year 1885–86, entitled *The Governing Passion* (1974), produced a new paradigm for the study of Victorian political life. They took as their guiding light Gilbert Pinfold's observation that in a democracy "men do not seek authority so that they may impose a policy. They seek

a policy so that they may achieve authority." Besides the obvious presence of Waugh, there is something rather post-Trollopian and neo-Namierite about a historical formula that denies to politicians (not to mention that extinct species known as statesmen) any abiding convictions or long-term goals. Every contender for office, they maintained, was prepared to use the Irish Question (among other questions) to rally enough support among centrist M.P.s to form a government. Contrary to the traditional view, represented by the late T. W. Moody, F. S. L. Lyons, and other non-Cambridge historians, who put too much trust in the spoken and written words of the politicians, the Pinfold school regarded pragmatism and the hunger for power as the controlling forces at Westminster. All ministerial claims to a coherent policy toward Ireland were relegated to the category of political posturing.

Andrew Gailey's book subscribes to Pinfold's precept. Gailey applies the "more sophisticated" concept of policy furnished by Cooke and Vincent to the period 1895–1905, when the Unionists were firmly in power and Anglo-Irish relations were being reshaped by the bitter quarrels within the Home Rule party after Parnell's fall. To his credit Gailey poses important questions about the contours and dynamics of British and Irish Unionism in these years. After reading his book, some readers may well want to enclose the keyword "policy" in quotation marks forever after. The author analyzes the two regimes responsible for Irish governance in this period—headed by the chief secretaries Gerald Balfour (1895–1900) and George Wyndham (1900–05). He interweaves the story of the "failures" of Unionist legislation for Ireland with a critique of progressive Unionism in Ireland. In effect, Gailey reduces "constructive Unionism" to a series of ad hoc maneuvers designed to keep Liberal Unionists happy within the coalition. He insists that the conciliation associated with the brothers Balfour as well as Wyndham had little to do with Ireland and everything to do with Westminster. For him the deep divisions between Parnellites and anti-Parnellites meant that the government did not really need to prescribe costly remedies to cure the Home Rule virus, because the latter had lost its virulence. And yet the Salisbury-Balfour administrations could not afford to neglect Ireland altogether because their existence depended on maintaining the Unionist alliance. Thus, even though the Home Rule "movement" had apparently disintegrated, both Salisbury and his nephew Arthur devised reforms in order to placate their Liberal Unionist supporters.

At the center of Gailey's book lies the argument that contemporaries and historians alike were conned into believing that Unionist "policy" was designed—in Gerald's famous phrase of 1895—"to kill Home Rule with kindness." Although Gerald added the qualifier, "*if we could*" (my emphasis), Irish nationalists denounced the whole idea as insulting, if not menacing,

to their cause. This oft-quoted trope serves as the fulcrum of Gailey's argument about the mythic nature of "Balfourism." According to the revisionist argument, Unionist attempts to translate this "kindness" into reform measures came close to destroying the very Unionism it was supposed to protect. Dismissing the idea that Unionist leaders had any "ultimate" goals for Ireland except to keep the country quiet, Gailey dwells on the primary importance of the Act of Union as the "ark" of the Unionist alliance, which had to be preserved at all costs.

To dismiss the goal of killing Home Rule with kindness as so much rhetoric or wishful thinking makes some sense, for few Unionists were foolish enough to expect Irish nationalist aspirations to wither away in response to sporadic and limited reforms. But to deny the Balfours credit for trying to "weaken" Irish appetites for autonomy by means of remedial measures is another matter. Gailey never really proves that the shapers of Irish "policy"—from the Balfours to Wyndham, MacDonnell, and Plunkett—did not have this goal in mind at some point. And to argue that remedial measures were designed solely for the benefit of Joseph Chamberlain, the Duke of Devonshire, and their followers is to ignore the role of such powerful pressure groups as the Irish Unionist Alliance and the Irish Landowners Convention, not to mention the Irish Unionist M.P.s led by Saunderson and Carson. Gailey's "all or nothing" scenario makes no allowance for the wishful thinking of Tories and moderate Unionists about the benefits to Ireland of remedial measures.

Gailey's book ends by taking Balfourian "kindness" to task, not just because it failed to kill Home Rule but because it "threatened to kill Irish Unionism" and "ultimately only secured the death of 'kindness' itself" (p. 319). This is a nice turn of phrase, but the post-mortem contains an element of overkill. Gailey denies the Cecils and their colleagues any credit for seeing an Irish dimension to their cherished formula of "the carrot and the stick." If the stick was not needed, then conciliatory carrots could be distributed among the inhabitants of John Bull's other island so long as the price tag did not raise too many Treasury hackles. By insisting that "between 1895–1905 Ireland was not home rule [*sic*]" (p. 312), Gailey marginalizes the impact on Unionist sensibilities of the reunion of the Home Rule party under Redmond in 1900 and the disruptive activities of the United Irish League. This revisionist reading of Unionist "policy" rightly stresses the contradictions and the divisive effects of reform measures. But if the Balfours really came as close to killing Irish Unionism as he contends, then this "cause" must have been far more fragile than most historians—from Buckland to Bew and Gibbon—have hitherto allowed.

To argue that "Balfourism" also killed "kindness" amounts to a sentence of premature death on the material achievements of the Balfour-Wyndham regimes. If their reforms fell far short of a "new deal"

for Ireland, one should remember that the Treasury, not to mention British M.P.s, constantly circumscribed the extent of state intervention in the Irish economy.

What died after 1904 was not so much the idea of "kindness" as the cause of moderation in Ireland, both unionist and nationalist. The secret collaboration of MacDonnell and Dunraven had the unintended effect of shifting the center of Irish Unionism into the heartland of Protestant sectarianism and Orange intransigence. It also made most Redmondites wary of any attempts to design a "half-way house to Home Rule." Although Gailey's study of Anglo-Irish relations sheds light on Unionist maneuverings, the Pinfoldian premise still needs to be weighed and measured alongside Lyons's chapter, "Constructive Unionism," in his magisterial narrative history, *Ireland since the Union*.

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J. J. LEE. *Ireland, 1912–1985: Politics and Society*. New York: Cambridge University Press. 1989. Pp. xxi, 754. Cloth \$69.50, paper \$27.95.

Relying on his deep knowledge of Irish history and archival research in Ireland, England, Germany, and the United States, J. J. Lee provides a critical survey of Irish society and politics from the passage of the Home Rule Bill in 1912 through the signing of the Anglo-Irish Agreement in 1985. He attempts to point contemporary Irish historical scholarship in new directions, with less emphasis on Anglo-Irish relations—which have been a major concern for so long—and more focus on the Irish performance since winning sovereignty. Writing from a southern Ireland perspective, he analyzes and evaluates the potential and performance of the people and their leaders, both north and south, in the context of their cultures.

Lee sees Northern Ireland as a striking economic failure in the twentieth century and the republic's economic growth as the slowest of any European nation during this period. He compares the Irish republic's economy with that of other small European nations that emerged after 1918—many of which have performed better despite more serious losses and deprivation. He finds John Kelleher's view of Ireland in 1957 still valid today, to wit: "one can hardly avoid deciding that Irish ills are largely psychosomatic" (p. 527).

For Lee, Ireland has not made wise use of its resources, both human and otherwise, and sadly lacks a "performance ethic." It has relied too heavily on imports, both intellectual and industrial, while hoping in vain for native industries to develop. After centuries of foreign domination, the Irish absorbed that with which they were most familiar. They failed to analyze or question as they adjusted to their new

circumstances after independence. Their acceptance of the English model limited their perspective and their leaders have learned little from history. They have ignored European experiences that could help them overcome their "tunnel" vision and synthesize various problem-solving policies.

Overemphasis on technology as a cure for all of Ireland's problems has created a problem in itself, in Lee's opinion. He believes that Ireland must have a "firm intellectual infrastructure in the human sciences" before it can make significant economic progress, but he is not sure that the Irish understand this concept. He sees the Irish as unreceptive to intellectual analysis in the media or elsewhere, a mindset that cannot be blamed solely on the church. For Lee, the church's traditional hostility to independent thought reflects forces at work in Irish society itself. Ireland's progress has also been limited by its peoples' willingness to accept and to maintain standards of lesser quality, both in public leadership and in daily life. Lee urges in-depth studies of Irish society to determine why this is so.

The work of the New Forum, which led to the Anglo-Irish Agreement, does not escape criticism. Lee faults it for an occasional romantic view of history and for its failure to address the reality of Ireland's performance since independence and the quality of its leadership, both North and South. He sees the Anglo-Irish Agreement as an indication that Britain realizes partition has not worked, but he acknowledges that northern and southern Irish leaders have not inspired the public confidence necessary to forge a united nation. He appears to question whether this can be achieved, in view of the tensions within Irish society that have created what he calls a "begrudging" approach to life, an envious, spiteful approach that has been projected onto Irish institutions.

Lee is not without hope, but, until a solution can be found that guarantees dignity for Unionists and Nationalists alike, he believes that stability in Ireland must remain a British interest. Many Irish and Irish Americans may not appreciate this thoughtful analysis, but it should be read and pondered. The book is needed and could have been written only by one of Ireland's own.

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DUNCAN TANNER. *Political Change and the Labour Party, 1900–1918*. New York: Cambridge University Press. 1990. Pp. xvii, 504. \$65.00.

This is the latest contribution to an immense and still growing literature concerning the replacement of the Liberal party by the Labour party. It takes an original line. Hitherto, speaking broadly, there have been two conflicting interpretations of that important evolution in the history of British politics: that the Liberals

were doomed once Labour appeared on the scene because they could not compete with the new party for working-class support and, alternatively, that World War I killed a party that otherwise might well have survived Labour's challenge. Providing a "contextual, disaggregated and interactive view of electoral politics" (p. 13), Duncan Tanner maintains that neither interpretation is satisfactory, although the impact of World War I was important. Even in 1918, however, the eclipse of Liberalism by Labour was not inevitable, although it had become likely.

Tanner's essential argument is that, until 1914, political anti-Conservatism in Britain took the form of a progressive alliance in which Liberalism was the senior and Labour the junior partner. Had the general election, which was due sometime before the end of 1915, taken place as scheduled, there is no reason to think that this relationship would have altered. Indeed, according to Tanner, there is every reason to think that it would have continued, regardless of the election's outcome. World War I, however, produced a situation in which the progressive alliance no longer made sense. The general election was postponed (it took place, finally, in 1918), and in the meantime war conditions had transformed the political scene. It had strengthened the appeal of a "Labourist" party advocating statist policies, while splitting the Liberals between "Wee Free" followers of H. H. Asquith and "Coalitionist" followers of David Lloyd George, while simultaneously undermining their political identity as reformers. Even so, the end was not preordained. During the early 1920s, Liberalism retained significant strength in parts of the country. Thus, Tanner would welcome a further study dealing with the period 1918–31.

It may be said, first of all, in assessing this study, that few works have been researched more assiduously. Tanner has mastered an extensive secondary literature dealing not merely with the rise of Labour and the fall of Liberalism but also with most aspects of Labour and Liberal history during the decade-and-a-half that preceded World War I. His survey of the parties' relative strengths and weaknesses—ideologically, socially and politically, and at the center and local level—during this period is almost obsessively detailed. Systematically he examines every constituency in the Northwest, the Tory regions, the coal fields, the heavy industrial heartlands, and Yorkshire. His conclusion that the progressive alliance still made political sense in 1914 and that the Liberals were then still the main anti-Conservative party is based on the findings of this survey and scarcely seems open to doubt. Henceforth, it will be impossible to argue, as George Dangerfield did, that Liberal England was on the ropes in 1914. Clearly the opposite is true.

Tanner's second conclusion, that World War I killed the progressive alliance but not the Liberal party is, perhaps, less certainly established. To have fully substantiated it he would have had to continue the systematic analysis of constituency politics during

the 1914–18 period. Instead, he offers less exhaustively researched (and less original) explanations. For example, not only were the Liberals split, and Labour's statist approach vindicated by the war, but Asquith had been enfeebled, presenting Arthur Henderson with an opportunity he was quick to exploit. Tanner does examine carefully the impact of franchise and boundary reform in 1918, but discounts the former as a factor in Labour's rise. The latter, he thinks, played a role, but not a decisive one. Still, historians will disagree with Tanner's interpretations here only after much thought. He knows the territory awfully well.

Perhaps he knows it too well? In labeling a variety of politicians and political attitudes, his distinctions between New Liberals, Old Liberals, Radical Liberals, radical Liberals, Centrist Liberals, and Liberal Imperialists, among others, remains unclear. The same is true with the Labour party: what was the difference between a Statist and a Collectivist, between the ethical socialism of some left-wing Labourites and the ethical collectivism of some radical Liberals? What, for that matter, was the difference between Tory collectivism and Labour collectivism? Not all these categories seem useful. In the section on constituency politics maps would have been helpful. Finally, Tanner's use of the word "spatial" when he often means "geographic" is irritating.

These are, however, minor matters in comparison with the achievement of the work as a whole. Tanner has made a signal contribution to the important debate on the fall of Liberalism and the rise of the Labour party.

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BILL LUCKIN. *Questions of Power: Electricity and Environment in Inter-War Britain*. New York: Manchester University Press; distributed by St. Martin's. 1990. Pp. viii, 200. \$49.95.

Historians of technology understand that the development and adoption of technologies are bound to prior economic, political, and cultural commitments. This book lies at the intersection of technology and the rhetoric of cultural values. Bill Luckin presents only the rhetoric of those values and judges their social intent without reference to the underlying technical, political, economic, or social realities.

Historical studies on the development and introduction of complex technological systems are few, especially for Britain. This study is one of the first on the expansion of the domestic electrical supply industry in the interwar years. It represents a great deal of archival work and relies heavily on the secondary literature on American, not European cases. It is clear that cost, desecration of the landscape, and pollution are old issues in the arguments about this technology.

The narrative is divided into "Adoption" and "Re-

sistance." But, despite the author's claim that "opposition to technological change" has "lain at the heart of British social life" (p. 1) in the last century and a half, the issues narrated here are not of technological change but its implementation. The narrative in "Adoption" leaps from a discussion of the propaganda for adoption ("technological triumphalism") of the electrical industry in advertisements, pamphlets, and films, to passionate discussions within the Manchester City Council over the cost of electricity versus gas for heating in working-class housing, not electrical supply itself.

"Resistance" is not to the expansion of supply but to pollution and the location of the hardware of the grid system (pylons and high-tension wires): pollution caused by the location of the Battersea Power Station, desecration of the landscape, and the destruction of rural England in the expansion of the grid system. The same rhetorical ground is covered in an afterword on nuclear power.

The author glides over but does not explore issues in the history of domestic technology, the reluctance of even the Labour party in this era to see electrical supply as a social service, the struggle over central government power versus local control, and issues in intellectual history. Also, it is up to the reader to know about the awful muddle behind electrical supply in London, the previous struggle over the electrical industry there, the growing powers of the London County Council, and the background of the conservation movement in England before 1900. The reader also needs to know the "units" for electrical rates (presumably a kilowatt hour) and domestic gas supply (presumably the therm). Luckin does not indicate their energy equivalence, trivial for heating, so that the relation between rhetoric and reality remains undefined. To go beyond narrative and to judge social meanings requires this next step. For example, the author's contention that the industry's rhetoric spoke to the middle class and up (true) and ignored the working class is propaganda, for the ideology of the capitalist bourgeoisie simply cannot be judged. Luckin also claims that the working class could not afford the service until the late 1930s, the time when the industry finally does take note of it. It may be that the industry was target marketing, not socially manipulating their audience.

The author's sympathies lie with the rhetoric of "Resistance," although this brings in problems. Although Luckin displays deep sympathy with a rural way of life destroyed after World War II, he describes the movement to preserve it and its landscape as led by those privileged few (including the historian G. M. Trevelyan) who wanted the landscape for only the privileged few.

Given the topics covered, the sympathies of the author, and the afterword, one senses that the volume is an addition to the current debates over the

electrical supply industry in England rather than a historical exploration of its expansion in the 1930s.

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DAVID THOMS. *War, Industry, and Society: The Midlands, 1939-45*. New York: Routledge. 1989. Pp. x, 197. \$62.00.

This book, which began its existence as a doctoral dissertation, is far narrower in its focus than its title would suggest. It deals only with two matters: the expansion of the aircraft industry and, briefly, the effect of air raids in Birmingham and Coventry. Although there is, to be sure, some connection between these topics as David Thoms handles them, they constitute two almost entirely different stories. Moreover, the research is severely limited to this pair of cities. There are no comparisons between the experiences of Birmingham and, for example, Bristol, as aircraft contractors, or between the reaction in Coventry to the great raid of November 14, 1940, and the greater suffering experienced in London. There is no introduction nor any explanation of why these two cities are unique, although one may suspect that they were chosen because the author happened to do his graduate work in the area.

That being said, much in this slim volume rewards the investigator of the civilian side of World War II. Thoms writes from the point of view of the industrialist, rather than the politician. The excellent Civil History Series has already dealt at length with British war production as seen from Whitehall. Here we discover from a meticulous survey of company records what industrialists, accustomed to managing their firms as they chose, encountered in having to deal with the Ministry of Aircraft Production (MAP) and the Ministry of Supply. Supply, it may be recorded, was far more rigorous and businesslike than the MAP, which, despite the innumerable pats on the back collected by Lord Beaverbrook, was found to be carelessly and badly run. In the MAP's defense, however, the author notes that it inherited a defective organization from the Air Ministry. Until the arrival of Winston Churchill, the Air Ministry had attempted to do its own contracting with inadequate knowledge of the efficiency and varying capabilities of the new firms, mostly automobile makers, seeking to enter aircraft production.

In addition to the problem of inexperienced government, industrialists had to deal with the problem of untrained and unschooled labor. The huge new "shadow factories" (works built at government expense in conjunction with an existing plant and sharing management) required tens of thousands of workers. Here the nation came to learn the cost of the Great Depression. Labor was, to be sure, growing scarce in the Midlands, although in December 1940 at

the height of the Blitz, nationally over six hundred thousand men and women were still drawing unemployment benefits. Far too many of the workers drawn into employment by the massive expansion of the aircraft industry in 1939 and 1940, however, had never in their lives known sustained employment. The discipline and punctuality demanded by factory scheduling was foreign to them. An immediate consequence was that increased wages and bonuses produced not harder work but absenteeism. One could comment that this paradox might have been foreseen; shipyards in World War I encountered the same problem. There may be some confirmation here for Correlli Barnett's controversial thesis in *The Audit of War* (1986) that even in the desperate emergency of Adolf Hitler's war, British industry was far less efficient in terms of worker utilization than German industry. The production miracles of 1940 and 1941 were accomplished simply by pressing into the factories endless numbers of workers.

This volume is an unpolished but useful book. It examines a critical and central aspect of Britain's war effort from the viewpoint of the boardroom and the shop floor. A longer, comparative, and more finished study would be of great value.

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Chicago

MARK MOTLEY. *Becoming a French Aristocrat: The Education of the Court Nobility, 1580–1715*. Princeton: Princeton University Press. 1990. Pp. x, 241. \$39.50.

This brief, intelligent monograph analyzes the "education" of the sons of the French high nobility in the period from the religious wars to the Enlightenment, using an impressive range of published memoirs, private papers, and other descriptive sources. The subject is education in the broadest sense, concerned more with strategies of cultural adaptation than with schools, books, and programs of study. Mark Motley follows the young nobles through infancy (the care of a governess), youth (governor and tutor), school (*collège*, academy, school for pages, royal guard regiment), and entrance into the world (travel, introduction at court, exposure to battle). He thus illuminates a number of unfamiliar corners in the lives of a group whose function in collaborating with or resisting the monarchy is central to the reevaluation of French absolutism.

He rejects both the traditional idea that the nobility turned to education in order to compete with the rising professional classes and the more recent suggestion that they developed the notion of racial superiority as a defense mechanism. His nobles inherited a deep-felt belief in their hereditary distinction, but they consciously adapted their skills to fit the demands of a changed social environment, and they did so in distinctively aristocratic ways, even while

sharing to some extent in society's new attitudes toward children, religion, and self-discipline. Motley revisits the family and finds parents who cared about their children and faced sobering problems of discipline. As the tutor of a sixteen-year-old La Trémoille charge wrote, "the bigger he gets the more problems he causes, and he won't stand for being reprimanded, even in his bedroom or other private places. When I try, he . . . asks why I'm wasting his time" (p. 85). Parents to whom this sounds familiar might consider that in the seventeenth century teenage sons also had the power to set off permanent feuds between clientèles, anger the king, or kill each other in duels. Avoiding such pitfalls required special training. Families employed governors and placed their sons in noble households so that they could acquire elegant speech and graceful comportment while learning the subtle rituals of aristocratic sociability. They sent them to *collèges* to learn to read and speak some Latin, but they lodged them in special quarters and provided extra training in French, history, and math to prepare them for lives as courtiers and army officers. Early departure after one or two years assured that minds of cadets would not be warped by the excessive book learning given to children of the professional classes. Noble academies—here discussed fully for the first time—could then polish riding, fencing, and interpersonal skills. Alternatively, the school for pages—restructured by Louis XIV and also introduced here—could provide access to the proper courtly rituals. Travel and military service added perspective and provided further social contacts.

These largely unknown experiences suggest nothing less than the redefinition and perpetuation of a social class. Unfortunately, Motley only hints at aspects of this interesting process. His anecdotal evidence is not always rooted firmly by date, making it difficult to tell how much things changed between Henry IV and Louis XIV. He shows us how the *grandes* survived, but not what that meant for the nobility as a whole. The gulf he sees growing between his families and the robe calls into question the assimilation of robe and sword, but the implications of this finding are not fully explored. Discussion of the training of sons destined for the clergy is minimal, and the lesser nobility is understandably left out. This book, then, opens promising new territory and raises stimulating questions without yet providing any definitive answers.

WILLIAM BEIK
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JEFFREY W. MERRICK. *The Desacralization of the French Monarchy in the Eighteenth Century*. Baton Rouge: Louisiana State University Press. 1990. Pp. xiv, 196. \$27.50.

Jeffrey W. Merrick explores an important change in eighteenth-century political culture. He demonstrates

that from 1714 on the French monarchy's connections with religious belief and practice weakened and a secular conception of government emerged; religion ceased to seem the necessary foundation of an effective polity. Thus, the culminating moment in his study comes in 1787, when the crown declared that non-Catholics could enjoy full French citizenship, although not full exercise of their religious beliefs. This book therefore contributes substantially to recent efforts to reinterpret the French revolution in terms of changing political values and assumptions during the previous century.

Merrick seeks to chart the course of desacralization and to provide explanations for it, and for explanations he turns mainly to the century's religious conflicts. Jansenists, he argues, mattered more than philosophes in changing French political culture. Insiders' disputes about traditional questions counted for more than attacks by intellectuals, who could only view political life from the outside. Merrick gives most of his attention to the bishops, who sought to destroy Jansenism, and to the magistrates of the parlements, who sought to protect it. In between stood the crown, hesitantly trying to reconcile the antagonists and, failing in that effort, insisting on a silence that had its own radical implications. If the crown accepted perfunctory adherence to external practices as sufficient evidence of orthodoxy, what had become of its obligation to defend the Catholic faith? And what had become of religion's usefulness in creating obedience? There was worse, for both bishops and parlements deployed rhetorics that challenged royal authority. The magistrates especially developed a vision of the state as founded on natural rights and laws, and having little to do with religion. Jansenism in fact shaded easily into republicanism for it "located authority within the Christian community in the body of its members," rather than in hierarchical superiors (p. 117).

Merrick documents these changing views of the polity with remarkable research among the century's pamphlets, sermons, and legal commentaries, but his work raises several questions. It closely parallels that of Dale Van Kley and other students of eighteenth-century Jansenism, from whose views he does not attempt to differentiate his own. More important, Merrick's rich descriptions of eighteenth-century political rhetoric leave questions of explanation unanswered. After all, bishops and *parlementaires* came from the same milieus; how are we to make sense of violent ideological differences within a socially cohesive ruling elite, and how seriously should differences be taken? Jansenist beliefs seem insufficient explanation for the passion that magistrates apparently brought to attacking the king's religious policies. Jansenist magistrates had been quiet between 1660 and 1714; what made their successors so ready to fight as the eighteenth century opened?

Like much recent research on eighteenth-century political culture, Merrick rejects social explanation of

such pugnacity in favor of political narrative. He recounts the great Parisian struggles of the eighteenth century, restoring to them their long-neglected religious dimensions. Yet in this case the narrative carries insufficient explanatory power, for political tensions seem to have been high at the very start of the century. The book gives us a fine understanding of the ideological terms in which tension was expressed, but leaves us uncertain about its sources.

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HANS-JÜRGEN LÜSEBRINK and ROLF REICHARDT. *Die Bastille: Zur Symbolgeschichte von Herrschaft und Freiheit*. Frankfurt: Fischer Taschenbuch. 1990. Pp. 335. DM 19.80.

The two authors of this excellent book are now recognized masters in the German school of historical studies of culture. The German school has emphasized analysis of the social and cultural contexts of concepts, keywords, and symbols, and especially audience reception and the reworking of them over time. This study focuses on one enormously influential collective symbol, the Bastille, and examines its development as a symbol from the eighteenth century to the present. The argument falls into four parts. The authors first trace the genesis of the Bastille as a political symbol from 1715 to 1789, arguing that only in the eighteenth century did the fortified prison in eastern Paris become synonymous with despotism. This development was surprisingly paradoxical: critics succeeded in identifying the Bastille with monarchical despotism just when the state stopped using it for punishment of its critics. The number of prisoners in the Bastille had dropped precipitously by the 1780s, hardly anyone was incarcerated by that time for criticism of the regime, and the treatment of prisoners had improved dramatically. There is no better example than the Bastille of the great power of a literary myth.

The account of the origins of the myth sets the stage for the second and most important part of this study, the transformation of the attack on the Bastille in July 1789 into a world-historical event. The authors argue that the reporting and commentary on the event in new newspapers and ephemeral literature made the event into a symbol of popular sovereignty and popular justice. Contemporary writers and printmakers cast the event as the moment of the birth of freedom and the inauguration of a new era. The celebration of the heroes of the assault, the idealization of the few prisoners found in the Bastille, and the demonization of the fortress commander, the Marquis de Launey, all contributed to the process of making a powerful icon.

In the third part of the book, the authors describe the uses made of the Bastille during the decade of

revolution. They consider this an example of the process of revolutionary "self-mystification," that is, a central means for creating a revolutionary myth and a new national political culture. The Bastille appeared in children's toys, ceramics, clothing, medals, songs, plays, festivals, and in memoirs of the heroes of the assault. Most fascinating in this section is the account of the entrepreneurial activities of Pierre-François Palloy, who made a business out of relics of the Bastille and even organized a procession of the dismantled stones of the prison around France.

The fourth and final part of the book analyzes the accounts of the Bastille in nineteenth-century historiography and the uses of the Bastille symbol as a weapon in the republican political arsenal, in the resistance against Nazi occupation, and as a continuing rallying cry for oppositional groups in the present. By the 1980s, however, the Bastille had become as well a truly national symbol of consensus. In 1989 polls showed that the French considered the storming of the Bastille the number one event of the French revolution.

This book is a model case study of how verbal, narrative, graphic, theatrical, and plastic-arts modes and means could transform a place and an event into a potent political icon. The authors go beyond the usual semiotic analysis of political culture, which too often remains centered exclusively on texts, to consider the various practices and even the business of sign making. Yet, despite the authors' skill in uncovering the processes of meaning formation, the Bastille remains an almost mysterious force. When it "fell" in July 1789, everyone—in France and in the rest of the Western world—almost immediately apprehended its far-reaching significance. In many ways, the commentators were simply recognizing and giving shape to a feeling already widely shared. In other words, the Bastille shows not only the importance of the political, social, and cultural contexts of symbol formation but also the surprising and still unfathomable ways in which an event, a collective action, with its effervescent unpredictability, can transform and disrupt all settled expectations.

LYNN HUNT
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DORINDA OUTRAM. *The Body and the French Revolution: Sex, Class, and Political Culture*. New Haven: Yale University Press. 1989. Pp. ix, 197. \$25.00.

For its bicentennial, the French revolution has taken quite a beating from academic chroniclers and commentators. In France, England, and the United States the mood was generally revisionist: François Furet and his many followers, while praising the revolution's liberal beginnings, deplored its subsequent drift into repressive dictatorship. Further to the right, Simon Schama and others argued that the rot had set in as early as July 1789, as middle-class leaders tacitly

condoned the violence that accompanied the taking of the Bastille. But criticism has also come from other quarters. Recent feminist analyses of the revolutionary process in France, such as Joan Landes's influential *Women and the Public Sphere in the Age of the French Revolution* (1988), point to the discrepancy between the revolution's universalist claims and its conscious, systematic exclusion of women from public life.

Dorinda Outram's challenging study of the French revolution's "body politics" in part echoes these feminist concerns. But the author also extends her argument to encompass male as well as female bodily concerns, and class-based differences in perceptions of the body and in physical demeanor. The revolution, she argues, was led by a male bourgeois political class whose physical ideal was one of bodily restraint and passive stoicism in the face of death. This middle-class male ideal of physical self-containment prevailed over and silenced other forms of bodily expression—those of women and of the lower orders—and in doing so "weakened models for political change" (p. 163). The deficiencies of this dominant revolutionary culture, as reflected in physical demeanor and images of the body, are connected to the political culture of modern totalitarianism with its focus on the charisma of leaders and physical repression of marginal or dissident groups.

This is a densely argued book bristling with fresh insights and provocative arguments. Outram takes pains to distinguish her methodological approach from that of other cultural historians such as Michel Foucault and Furet. She takes issue with the premises underlying their works, arguing forcefully for the need to reintroduce questions of individual intention and volition into the history of the revolution. The problem with history-as-text is that it collapses all discourses into expressions of the same episteme or semiotic circuit; only through the analysis of intentions can we account "for the diversity of the repertoires of political action in the Revolution, or for the rapid changes in these repertoires" (p. 36). The rationale for a history of the revolutionary body is that "the body is the space where intentionality and episteme can come together" (p. 36). Thus, Outram purports to give us not a history of the ways in which bodies were defined by a cultural context but instead an account of the creation of a political culture through the willful manipulation of body politics.

The five chapters in which Outram unfolds her argument are linked by a common theme derived from Norbert Elias's thesis concerning the parallel formations of the modern state and of the characteristic demeanor of *homo clausus*: the autonomous, self-controlled, physically "civilized" individual. But because Elias's argument ends with the apogee of court society in the late seventeenth century, Outram proposes to carry it into the revolutionary era. The men of the revolution, she suggests, cultivated stances of stoic self-sovereignty and rigid virtue "as the means

by which their seizure of power from the monarchy could be validated and the new state strengthened" (p. 82). These men, whom Outram resolutely characterizes as "bourgeois," cultivated the demeanor of *homo clausus* as a sign of difference from other classes and from women. In a related chapter, the author argues that the ideal of physical self-control explains both the invention of the guillotine and the frequent recourse to "heroic suicide" among revolutionary leaders. Such means of dying served both to avert middle-class fears of loss of physical control in the face of death while also contributing to displace bodily expressivity onto women and working-class revolutionaries.

Outram is one of the few cultural historians to have made use of gender as a category to explain the behavior of men as well as women; in fact, her chapters on male bodily ideals and attitudes are much more original than the single chapter she devotes to a woman, Madame Roland. Outram also takes an unconventional approach to the debate about the social bases of the revolution. Her uncritical use of the term "bourgeois" to define the revolution's leaders would seem at first to align her with Marxian orthodoxy; in the conclusion, however, she clarifies her argument by explaining that she defines class, following Pierre Bourdieu, as an entity "located by the multiplicity of its practices, in their interrelationship rather than their specificity" (p. 153). Outram remarks that only a definition of a class as "being constituted" can fully integrate questions of subjectivity and self-definition. One only wishes that she had made this premise clear at the outset.

The book's greatest virtue is that it is framed by a large and important question derived from, but left unanswered by, the work of Elias: if, as Elias argued in *The Civilizing Process* (1939, 1982), the ruling classes of sixteenth- and seventeenth-century Europe, under the influence of royal courts, learned increasingly to control their physical functions and to restrain their aggressive urges, how and why, at the time of the French revolution, was violence definitively transferred into the hands of the state? To this difficult question Outram brings at least the beginnings of an answer.

There is, of course, plenty to disagree with in Outram's book. Its essay-like structure makes for repetition while obscuring crucial links in the author's argument. Outram's determination to use the French revolution as a means to explain the origins of the fascist state not only strains her argument but also places her, as she admits, in "that interesting and dangerous area where conservatism and the left join each other" (p. 26). Finally, some will find the author's tone excessively pugnacious, especially since she tends to be most critical of precisely those thinkers (Foucault, Furet, and Lynn Hunt) whose writings laid the groundwork for her own work. But, quirkiness aside, this book is a timely, original, and stimu-

lating contribution not just to its field but also to cultural history in general.

SARAH MAZA

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PAUL R. HANSON. *Provincial Politics in the French Revolution: Caen and Limoges, 1789–1794*. Baton Rouge: Louisiana State University Press. 1989. Pp. xviii, 273. \$32.50.

Two visions of republicanism collided in France during the spring and summer of 1793. Divisions between the Jacobins/Montagnards and the Girondins/Federalists deepened as a result of the trial of Jean-Paul Marat and the formation of the Commission of Twelve and culminated in the proscription of twenty-nine Girondin deputies from the National Convention on June 2. Civil war between the two factions soon broke out.

In this work, Paul R. Hanson discusses why some regions of France adhered to the Paris government while others openly revolted. Comparing provincial politics in Caen and Limoges enables him to analyze the larger regions surrounding these urban centers. Hanson explains why Caen, a Federalist stronghold, refused to recognize the authority of the Paris government after the purge of June 2 while Limoges supported the Jacobins. He also shows how these two localities reacted to events in Paris and, in some instances, actually influenced political developments in the capital.

Hanson selected Caen because it is the only one of the four centers of Federalist insurrection, which also included Bordeaux, Marseilles, and Lyons, that has not been extensively researched. Limoges was chosen for comparative purposes because it was similar in size and administrative functions. Each was a seat of an intendancy and an important regional court, and each was a trade center in its area. Neither city hosted a *parlement*. The two differed in that Limoges was an episcopal city (Caen was not), Caen had a sizable Protestant minority (Limoges did not), and Caen was in a rich grain district (Limoges was not).

Despite their similarities—or perhaps because of their differences—Caen espoused the Federalist cause while Limoges supported the Jacobins. Why? Hanson conducted a close examination of the local records to gain an understanding of the social and economic structures of the two cities. His well-documented study lends support to the thesis of Edward Whiting Fox, Alexis de Tocqueville, and others who have argued for a political dichotomy between the "agricultural hinterland" (north, east, and central regions) and the "commercial periphery" (west, south, and coastal regions). Hanson includes maps showing that the areas that supported or opposed Paris in the civil war corresponded closely to this division. This conceptual framework also applies if one uses Tocqueville's distinction between *pays d'élec-*

tion (Limoges) and *pays d'état* (Caen). Caen (commercial periphery) joined the revolt in 1793 because of its prosperity and the political and economic control exercised by its commercial elite, as well as because of its opposition to the steadily increasing encroachment of the central government. Limoges (agricultural hinterland), a trade and administrative town (but much less prosperous than Caen) without a dominating commercial elite and more accustomed to accepting the intendant as the leading political figure, backed the Montagnards. The reactions of Caen and Limoges to the events of 1793 go far to substantiate Fox's contention that the Federalist revolt "represented the last stand of the old commercial society against integration in the administrative state."

Hanson demonstrates that local politics in Caen and Limoges had some influence on national events, thus effectively modifying the view that localities merely reacted to events in Paris. As a result, he has admirably filled a void in our knowledge of the Girondin-Montagnard conflict and the Federalist revolt. Hanson's study will be indispensable to those attempting to understand the differences between two visions of republicanism that ultimately proved to be incompatible.

THOMAS F. SHEPPARD
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ARTHUR MITZMAN. *Michelet, Historian: Rebirth and Romanticism in Nineteenth-Century France*. New Haven: Yale University Press. 1990. Pp. xxv, 339. \$35.00.

Over recent decades most of the best work on Jules Michelet has come from scholars of French literature who have produced what Arthur Mitzman characterizes as "brilliant fragments" (p. xvi). As a historian, Mitzman advocates a fuller history of the individual. His book is a contribution of that kind: a biographical study that integrates a central part of Michelet's life and works within a broad context of French history. In a masterful way the author constructs that larger framework out of insights from recent writing on French political and social history, on popular culture and the "civilizing" efforts of elites, and recent thinking about gender.

But it is Michelet's historical writings and his inner life that dominate this study. Mitzman gives us a psychobiography, with homage paid to Peter Gay. In this account of the mature Michelet's thought, intellectual influences such as Giambattista Vico pale beside the relationship of the historian with his mother (who died when he was an adolescent). Guided by post-Freudian psychoanalytic theory, Mitzman zooms in on one period of Michelet's life: his crisis-ridden and creative mid-life, the fifteen years from about 1840 to 1855. During those years, Mitzman shows, the historian underwent profound changes in his self-understanding and ideology.

Michelet himself pointed out how intertwined his

life and works were. Paul Viallaneix and other specialists have already written much about the autobiographical sources of the historian's thought. Yet, with his understanding of psychological development and focus on primary relationships, Mitzman makes clearer than ever the stream of correspondences and connections between what Michelet lived and what he wrote. The author also shows well the tensions in Michelet's ideological positions—and his successive efforts to achieve harmony among them. In Mitzman's terms, the story is one of "returns" and "rebirths" following times of death: the death of his father, for example, and of a version of himself. Michelet made successive "returns" to childhood experience and retrieved from there emotions and insight that he brought to bear on current problems. Each "rebirth" yielded a reintegrated self with new ideological insight. By Mitzman's account, the keys to the changes were Michelet's new understanding of the maternal, the paternal, and the fraternal. A few years after the death of his first wife, he entered on a new life with his love for Madame Françoise-Adèle Dumesnil and came to a new acceptance of the maternal. Responding at the same time to France's crises of the 1840s, he embraced a leftist social romanticism in which sympathies for mother and nature were central. In 1848, Michelet, pained at seeing the revolution turn into fratricide, had to rework his revolutionary hopes. While he reoriented his private life around the love of the young woman, Athenais Mialaret, who became his second wife, as a historian he was writing about the death of revolutionary idealism in 1793—and was living through the death of the Second Republic. The dark chapters continue with the death of his infant son in 1850, the coup d'état of Louis Napoleon, the firing of Michelet from his two positions, and the death of his friend Félicité de Lamennais. The book concludes with his emerging to new life in 1853–54, when he restored his health in Italian mud baths (in "the maternal embrace of the earth" [p. 276]) and conceived the hopeful, visionary book *Le Banquet*.

Although the emphasis on such points as Michelet's "incestuous longing" (p. 53) for his mother may seem overdone to some, overall this book renders a sharp, fresh portrait. Other approaches are more successful in stirring appreciation of Michelet as a writer. Others probably better account for the mentality of a generation and social romanticism. The lengthy analysis tying thinking to early intimate experiences leaves the impression of an ideological genesis that was too particular and personal to be general to an era. But Mitzman's approach does succeed in making more understandable Michelet the man and his shifting views in mid-life. He does it by telling a complicated story exceedingly well while also providing plenty of close analysis and scene setting. It is a compelling story of a man grappling—intellectually, emotionally, creatively—with fundamental personal, social, and political problems. It is the history of a historian

whose unabashed subjectivity was fruitful and influential in ways his critics could not begin to imagine. Mitzman makes this concluding point by recalling how Michelet's work inspired Marc Bloch in the gloom of 1940 and how Michelet anticipated the *Annales* project of an encompassing, integrative history of humanity.

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RUTH HARRIS. *Murders and Madness: Medicine, Law, and Society in the Fin de Siècle*. New York: Clarendon Press of Oxford University Press. 1989. Pp. viii, 366. \$49.95.

The incongruity between the two great professional discourses—the legal and the medical—points to a key tension experienced by nineteenth-century Europeans in their efforts to make sense of themselves and their fellows. Legal discourse was predicated on a traditional belief in volition and moral responsibility; it deemed individuals guilty of crime insofar as they knowingly and willfully overstepped sanctioned limits. Medical discourse suggested that many individuals were not, properly speaking, actors at all but more akin to marionettes, their behavior determined by such factors as degenerative heredity, cerebral configuration, and neurophysiological automatism. The tension between these basic positions is still with us today. But it was felt more acutely a century ago due to the sheer novelty—and unprecedented popularization—of deterministic medical models at that time. Ruth Harris maintains that the uneasy commerce of law and medicine reached “a new and important turning-point” in France during the *belle époque*, when the balance of power shifted to medicine and the confident courtroom testimony of psychiatric experts routinely “undermined the traditional bases of the French judicial system” (p. 2). In her elegantly researched and richly textured book, Harris examines this late nineteenth-century medico-legal tangle.

The book is informally divided into two parts. The first four chapters provide an excellent synthetic account of relevant developments in penal and bio-medical theory. In the last four chapters, Harris shows how everyday practice in the Paris criminal courts operationalized this slice of intellectual history; how, in other words, medical argument was deployed in the judicial arena to bring about the punishment or exoneration of people who had committed violent crimes. She draws for this purpose on the voluminous records preserved in the Archives de la Seine, including the pretrial interrogations of defendants by the *juge d'instruction* (an institution unknown in Anglo-American law) and the proceedings of jury trials before the *Cour d'assises*. The translation of theory into practice is never a wholly straightforward matter, and Harris is especially interested in teasing out the

unspoken social norms that, in *fin-de-siècle* criminal cases, regularly intervened between the two. The norms that loomed largest, she holds, concerned gender and class.

Thus, by reference to more than sixty murder cases tried between 1880 and 1900, Harris shows that psychiatrists and magistrates alike were often quite comfortable with verdicts that absolved women of technical responsibility for their crimes. Female legal irresponsibility was such an acceptable concept, she argues, because it meshed well with the prevailing view of women as childlike, excessively emotional, and buffeted by the physiological events of menstruation and pregnancy. Hence the court readily assented to psychiatric claims that women had carried out crimes under the sway of hysteria or while hypnotized and obedient to the will of some dominant male. It readily accorded the non-medical label “crime of passion,” also entailing judicial leniency, to women who murdered faithless husbands and deceitful lovers. Women, Harris suggests, paid a price for this leniency. To be the recipients of it, they had to conform to the appropriate stereotypes of “good” female behavior: their rage at men had to be motivated by shame at the loss of virginity to a seducer or by concern for the welfare of their children and the sanctity of the marriage vow; they had to depict themselves not as autonomous beings with rights but as defenseless creatures needing male protection. *Criminelles* who adopted other styles were usually marked out for harsh punishment. The court's granting of leniency therefore had the effect of reinforcing the standard image of female inferiority and, as Harris aptly puts it, “underscor[ing] the inconsequentiality of female violence” (p. 241).

Working-class men who committed crimes under the influence of alcohol were similarly treated. They, too, were often spared the full weight of the law, but at the same time the court stereotyped them as infantile and tainted by a heredity that made drink irresistible. Bourgeois men, assumed to be more “adult” and biologically intact, did not get off so lightly.

In confronting the almost limitless empirical detail contained in these court records, Harris has avoided facile interpretation and employed multiple interpretive strategies. Especially intriguing is her analysis of legal testimony in terms of literary genre; both defendants and professional men, she argues, unself-consciously adhered to the conventions of melodrama when describing the female *crime passionnel*. Harris's tendency, however, to find things ambiguous can get frustrating. When, for example, she tells us by way of conclusion that the medical and legal viewpoints were neither as far apart as is usually thought nor “identical” (p. 321), the reader has the impression that the author is straining against her own analytic framework. Yet this remains throughout a stimulating and imaginative book that should command the attention

of cultural, social, and gender historians as well as historians of science and medicine.

JAN GOLDSTEIN
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PETER H. AMANN. *The Corncribs of Buzet: Modernizing Agriculture in the French Southwest*. Princeton: Princeton University Press. 1990. Pp. xiv, 292. \$39.50.

LEO A. LOUBÈRE. *The Wine Revolution in France: The Twentieth Century*. Princeton: Princeton University Press. 1990. Pp. xiv, 288. \$29.95.

Although World War II ended forty-five years ago, that period has more often than not been considered current events rather than history. These two books begin to reclaim the period for historians, and they show both the promise and the pitfalls of attempts to write a social history of recent times.

Leo A. Loubère's book extends into the twentieth century the history of the French wine industry that he began in his *The Red and the White* (1978). An introductory chapter outlines his story: the problems of destruction and lack of a skilled workforce during World War I, overproduction, depression and legal reforms between the wars, mechanization and increased productivity after 1945. Subsequent chapters take up different aspects of the wine industry within this general chronology and in doing so flesh out the principal features of what Loubère believes is a revolution. One aspect is the mechanization of the vineyard, especially the adoption of tractors in the 1950s (not just for plowing but also for pruning and spraying), as the focal point of a new viticulture. Yet another feature lies in changing methods of wine-making. Mechanization had also come to this area, with machines for pressing and bottling. A better scientific understanding of the making of wine also allowed increases in productivity while improving the quality of the wine.

One of Loubère's major themes has to do with the struggle against fraud. The centerpiece of this is the *Statut de la Viticulture*, a product of falling prices during the depression that made the system of *appellations* the mechanism for guaranteeing quality for the consumer and income for the producer. This also had the effect of bringing the state bureaucracy into the process of producing and marketing wine, a process that has continued since 1945, creating "more laws than the average vigneron can read in a lifetime" (p. 105). These developments were related to the changing economics of wine: periods of prosperity for fine wines, above all in the 1960s, and improved marketing, which helped to finance the purchase of machinery. The development of cooperatives, which shared the costs of mechanization and contributed a growing share (47 percent by 1985) of wine production, allowed small-scale growers to move in the same direction. In the southern areas that produce common table wines, however, changes in consumption

habits and falling prices have created a continuing economic crisis.

The focus of Loubère's book is on technical, scientific, legal, and economic structures in the wine industry. The living conditions of those who work in this industry are covered in two final chapters. In wine-growing areas, as in other regions of rural France, the poverty of subsistence agriculture has given way to higher standards of living, increased mobility, rural depopulation, televisions, automobiles, and even disco. For Loubère, these developments have a tragic aspect, the destruction of the village community and the "creativity" that he believes was a part of it.

There is good reason why Peter H. Amann's book is named after one particular village. It is a collection of studies on five villages in the *département* of the Haute-Garonne in the period since the end of World War II, and chronicles the villagers' experiences with agricultural modernization. Most successful, and the obvious models in the book, were the farmers of Buzet-sur-Tarn. There, under the leadership of younger farmers and astute political leaders, sharecropping gave way to money leases and owner cultivation, landholding was consolidated and farms became larger, tractors replaced animals as motive power, and, after a brief foray into dairy farming, irrigated corn became a staple crop of a high-investment, high-yield agriculture. In the other villages, an array of material conditions, poor leadership, unsuccessful land consolidation, or missed opportunities made it impossible to compete in the "dynamic new society" of postwar France and Europe.

Amann describes the interplay of local concerns and national and international pressures that conspired to make agricultural modernization a success or failure in each village, and this makes the book a valuable reminder that large-scale patterns are ultimately the result of numerous local decisions and actions. These analyses bring out the role of local leadership, cooperative organizations, state intervention, and land tenure and quality in determining the ability of farmers to adapt to the new conditions of agriculture that have developed in France since the end of World War II.

These books have much in common. The postwar adaptation of French agriculture to market forces in both cases is above all a question of attitude, a rejection of traditional methods. Both books also emphasize the balance of power between generations in this equation: children would see their family farms modernized, or they would leave. The importance of mechanization, farmers' cooperatives, and state intervention in the history of French agriculture since 1945 follows. The authors also both work self-consciously within a conceptual framework of modernization. The effects of this are most noticeable in Amann's book, where the "failures" of Juzet-d'Izaut, Saint-Cezert, Cadours, and Castres are continually contrasted with the "success" of Buzet. Both also depend on oral history (Amann more than Loubère),

but more methodological self-consciousness is needed in this area: both consider oral information in the same way that historians typically critique written documents, but neither seems aware of current methodological discussions in ethnography. In spite of these problems, however, these two books provide us with an important beginning to the task of placing the twentieth-century French countryside in historical context.

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MANUEL GARCIA FERNANDEZ. *El reino de Sevilla en tiempos de Alfonso, 1312–1350*. Seville: Diputación Provincial de Sevilla. 1989. Pp. 358.

ISABEL MONTES ROMERO-CAMACHO. *Propiedad y explotación de la tierra en la Sevilla de la Baja Edad Media: El patrimonio del Cabildo-Catedral*. Seville: Fundación Fondo de Cultura de Sevilla. 1988. Pp. 501.

The two volumes under review are doctoral dissertations completed under the direction of the distinguished scholar, Manuel González Jiménez of the University of Seville. The starting point for both dissertations is the basic proposition demonstrated by González Jiménez, that is, that the Christian colonization of Andalusia in the thirteenth century following the fall of Seville (1248) did not succeed as the victors had hoped. Thus, the task of repopulation had to be undertaken once again in the fourteenth century. (See González Jiménez's groundbreaking studies, *En torno a los orígenes de Andalucía*, 2d ed. [1988], and *La repoblación de la zona de Sevilla durante el siglo XIV: Estudio y documentación* [1975]).

Isabel Montes Romero-Camacho studies the development of the rural landed property of the cathedral chapter of Seville from the initial endowment of the cathedral by Fernando III and Alfonso X in the thirteenth century until the end of the fifteenth century. This volume is a companion to her *El paisaje rural sevillano en la baja edad media* (1989), in which she discussed the evolution of the rural holdings of the cathedral chapter, its fields and buildings, and agricultural techniques.

The book under review is concerned mainly with the acquisition and administration of the rural patrimony of the cathedral chapter. Part 1 considers the formation of the patrimony and its development during four distinct epochs. The earliest extended from the fall of Seville to the end of the reign of Alfonso X. During that time the cathedral chapter received its first grants from the crown, but as Alfonso X's efforts at repopulation were generally unsuccessful, the position of the cathedral chapter remained precarious. A second effort at repopulation undertaken from the end of the thirteenth century to the end of the fourteenth had better success, so much so that the patrimony of the cathedral chapter grew rapidly, in part by the acquisition of lands held by

absentees. The full development of the patrimony took place from the late fourteenth century to the end of the first quarter of the fifteenth century. The remainder of the fifteenth century was a period of consolidation.

After the first burst of royal donations, subsequent acquisitions were due principally to private donors, both clerics and laypersons, usually members of the urban oligarchy. When gifts of property declined, the chapter continued to add to its holdings by exchanges and purchases. Properties acquired included towns, castles, villages, farms, vineyards, olive groves, orchards, mills, and so forth. Even though much of the rural area of the kingdom of Seville was brought under the control of lords pertaining to the rising nobility, there still existed small proprietors, chiefly adjacent to the city. The expansion of the chapter's patrimony was halted in the era of consolidation as a result of declining donations, reflecting an alteration of religious sentiments, and pressure from the urban oligarchy who competed for land more and more frequently with the chapter.

Part 2 of Montes Romero-Camacho's study deals with the exploitation and administration of the patrimony. The chapter was not engaged in the direct exploitation of its lands but rather contracted with others for that purpose. Larger and middle-sized holdings, usually devoted to the production of cereals and olives, were leased for life to clerics and members of the principal families of Seville. Such agrarian contracts were called *arrendamientos*. Small holdings, given over to vineyards, were often rented (*censo*) to artisans from neighboring towns and villages who used the profits from these lands to supplement their income. Although the cathedral chapter originally stood as a lord with jurisdiction over the peasantry who cultivated the land, in the late middle ages the chapter was transformed into a body deriving a substantial income from the different types of rentals already discussed. The chapter derived about one-half of its income from rural and urban properties and the other one-half from the tithe, which was essentially an impost on agricultural produce.

Montes Romero-Camacho has studded her work with numerous charts illustrating the rise and fall in the fortunes of the cathedral chapter. Graphically depicted is the evidence of acquisitions from donations, exchanges, and purchases, the comparative extent of vineyards and olive groves in different zones, and many other aspects of the chapter's use of its patrimony. Although there is not a separate bibliographical listing, the work is based on the rich documentation of the cathedral of Seville, which is fully discussed in her other book mentioned above.

By contrast, Manuel García Fernández concentrates on a more limited chronological period, namely, the reign of Alfonso XI (1312–50), and also on a more limited geographical area, the kingdom of Seville, one of the three Christian kingdoms in Andalusia. (The others were Jaén and Córdoba.) During

the late thirteenth century and the first half of the fourteenth century, the kings of Castile strove to gain control of the Straits of Gibraltar so as to prevent a Muslim invasion from Morocco. Although the kings of Granada were tributary vassals of Castile, they often took advantage of the Moroccan invasions to wage war against the Christians. By his victory at Salado in 1340, Alfonso XI eliminated once and for all the threat from Morocco, and by capturing Algeciras in 1344, he gained control of one of the keys to the Straits; but his death while engaged in the siege of Gibraltar left the war of reconquest suspended for another century and one half.

Given these circumstances, Alfonso XI realized the importance of providing for the defense of the region by the systematic development of a network of strategically located fortresses. The defense of the frontier also required that an effective repopulation be carried out, especially because the expulsion of the Mudéjars after their revolt in 1264 left many areas scantily settled. The nobility and the military orders played a significant role in the business of repopulation because military considerations were uppermost in the king's mind. The origin of the great lordships held by the nobility in Andalusia dates from this period of colonization rather than from the comparatively unsuccessful efforts of the thirteenth century. Despite the king's active interest, the population was never adequate, no doubt because the possibility of being plundered, killed, or enslaved by Muslim marauders was very real.

Although the king had only limited success in attracting settlers to the region, he effected profound change in the administration of the most important cities and towns that were directly dependent on the crown. Local oligarchies tended to predominate but their factional quarrels provided the king with the opportunity to intervene and to regulate municipal affairs to suit himself. The aim of royal policy was toward centralization and the subordination of local *fueros* to general territorial laws. Alfonso XI strengthened royal control over municipal government by replacing the open council with a closed council (*regimiento*) composed of persons named by the crown (*regidores*). Although a select group of townsmen drawn from the dominant elements in urban society constituted the closed council, its independence was limited by the *corregidor*, a royal official with final responsibility for the supervision of every aspect of municipal government.

García Fernández also discusses at length the economic foundations of the kingdom of Seville. Not surprisingly, the principal wealth of the region derived from agriculture. The land was given over mainly to the cultivation of wheat and other cereals, vineyards, and olive groves, but problems of climate, poor harvests, war, and inflation continually threatened those who tried to make their living from the land. Perhaps inevitably, considering the hazards of life on the frontier, small proprietors gradually

yielded their lands to the nobility or to ecclesiastical institutions. In this way, great estates began to emerge and become a dominant feature of the landscape. Although crafts were of modest extent and importance, the towns developed a significant commercial activity, linking them with the Muslim kingdoms of Granada and Morocco. Overseas trade provided a connection with the wider Christian world.

The society that emerged during the first half of the fourteenth century tended toward stratification, toward a fundamental division between privileged and nonprivileged groups. At the apex of the social structure were the nobility, divided into two principal categories, the magnates (*ricos hombres*) and knights (*caballeros hidalgos*). Some represented older lineages, but a new nobility of more recent origin was also becoming prominent. Within the towns, similar divisions were evident between the urban oligarchy composed of knights (*caballeros villanos*) and the merchants and artisans. Occupying a unique position were the religious minorities of Jews and the comparatively insignificant numbers of Muslims. The countryside was the home of peasants, some of whom were proprietors, others tenants of various sorts. Added to them were those unfortunates who were taken as slaves during the frontier wars.

García Fernández's book is well equipped with maps and charts, illustrating lines of defense, repopulation, lordships, the location of urban markets and shops, royal domain lands, and family relationships. Aside from the usual bibliography of printed works, he has also made thorough use of many municipal, noble, cathedral, and national archives.

Both works complement one another and portray the social and economic milieu in the kingdom of Seville in a most interesting and informative manner.

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JODI BILINKOFF. *The Avila of Saint Teresa: Religious Reform in a Sixteenth-Century City*. Ithaca: Cornell University Press. 1989. Pp. xv, 218. \$24.95.

There are a fair number of books about Saint Teresa in English, but not much is known about the walled city of Avila, her birthplace. By relating the saint and her reform of the Carmelite order to the religious and social changes that occurred in Avila, this work by Jodi Bilinkoff promises a new approach to a well-known figure. Although some chapters contain interesting material, I find the book as a whole not as good as the sum of its parts.

The years 1480–1520, covered in chapter 2, are of great consequence for religious reform in Castile, and one of the most influential agents of this reform, Francisco Jiménez de Cisneros, Cardinal-Archbishop from 1495 until his death in 1517, is accorded nothing more than a footnote. He appears briefly in chapter 4 (1540–70) in the context of his vigorous

campaign to have translated and published in Castile the works of Italian humanists and northern European devotional writers, but it is not until chapter 5 (1560–80) that we are told that Jiménez de Cisneros did “much to create an atmosphere receptive to various forms of spirituality and to new styles of religious life, open to both men and women” (p. 141).

Much of chapter 2 is concerned with the families who dominated Avila, and there is some confusion as to whether this dominant group, be they oligarchs, aristocrats, or elites, is entrenched or newly arrived. On page 22 we read that “in the late fifteenth century and early sixteenth century many urban aristocrats in Avila found themselves in the process of consolidating positions of power and influence only relatively recently achieved.” On page 26 we are told that “The composition of the city council remained amazingly stable, and essentially aristocratic, throughout the period. Analysis reveals continuity of family names to be a striking feature of this fundamental governing body.” Because neither the source nor the dates of this analysis are given, these contradictory conclusions remain unresolved. That Teresa’s well-known *converso* family, the Cepeda, managed to form marriage alliances with city councilors and their families in the first decade of the sixteenth century suggests that the aristocratic bias of the Avila oligarchy must have been somewhat diluted at that time.

Throughout the book the goals of spirituality are allied with social classes. Thus, those who dominated Avila in the late fifteenth century founded religious houses “to consolidate their positions within the city’s political and ecclesiastical structure and to ensure the perpetuation of their families’ power and prestige” (p. 39). By the mid-sixteenth century, this aristocratic bias was assailed by a reform party that “supported innovative religious institutions that served the social and spiritual needs of the urban community as a whole” (p. 201), including Teresa’s new nunnery, the Jesuits, and the college of the Niños de la Doctrina. This new spirituality flourished briefly until about 1580 when there was “a return to the traditional dynastic style of founding monastic institutions,” and “the city’s aristocracy reemerged as a major force in the establishment and control of religious institutions” (pp. 152, 159).

This chronology of spirituality is not entirely sustained by facts given in the book. Tucked away in the notes and text are pious foundations of the early sixteenth century that have little to do with family dynasties and are dedicated to meeting the new needs of an expanding urban center. The hitherto ignored Avila bishop Francisco Ruiz founded San Lázaro in about 1520. And the Hospital of the Magdalena, whose confraternity visited prisoners in jail, was established around 1511, while the Hospital of Santa Escolástica for sick, wounded, and abandoned children was established in 1509 by Don Pedro López de Calatayud, dean of the cathedral chapter.

We know that changes in spirituality occurred in

Spain and other Catholic countries from 1480 to 1620. The integration of Avila into the European scene would be better served by more discussion of the various forms of spirituality and religious life from 1480 to 1540, which might demonstrate that changes occurred earlier and were supported by a broad spectrum of society, including aristocrats. Nonetheless, by adopting an innovative approach to explore the background and achievements of a familiar figure, the book offers some challenging ideas.

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HELEN NADER. *Liberty in Absolutist Spain: The Habsburg Sale of Towns, 1516–1700*. (Johns Hopkins University Studies in Historical and Political Science, 108th series, number 1.) Baltimore: Johns Hopkins University Press. 1990. Pp. xvi, 305. \$39.50.

Helen Nader’s long-awaited new book is a masterpiece of the historian’s craft, in a neglected area. It is the first systematic study of the Spanish Habsburg rulers’ sale of townships (*villazgos*)—a policy of decentralization with far-reaching political and economic implications.

Nader had originally planned the book as a study of estate management by Spanish nobles. But she abandoned her original topic when she discovered that the nobles’ managerial activities were mainly limited to livestock and that most of the important decisions about land management and crop production were made not by noble lords but by municipal councils. Thus, Nader was led to a study of municipal government in Habsburg Spain. Historians should be grateful for this, because early modern Castilian urban history is an underworked area, particularly in the study of small towns and villages, where four-fifths of the population lived. The towns were the basic units of Castile’s territorial administration and controlled virtually all of the kingdom’s agricultural land. Historians often assert that Spain’s seventeenth-century decline was brought on by—or at least aggravated by—Castilian resistance to change. But Nader’s study depicts a dynamic and eagerly changing society, with autonomous local governments aggressively seeking ingenious ways to defend and increase their independence.

Spain’s Habsburg rulers, perpetually in financial difficulties, adopted numerous extraordinary money-making schemes. One of these was the sale of townships to subject villages in Castile who wished to escape the jurisdiction of their ruling cities or towns. The result was that the proportion of autonomous towns nearly doubled: whereas only about 40 percent of Castile’s municipalities were towns before 1500, by the end of the Habsburg period over 75 percent had been elevated to “town” status.

The royal government profited from the sales—not merely from the immediate proceeds but also from

increased royal tax-collecting ability through direct negotiations with ever-more-numerous independent municipalities. Politically, the result was a progressive decentralization of power. But the newly autonomous towns proved to be intensely loyal to the monarchy that had allowed them to gain their administrative freedom. And they exercised control over local resources so efficiently that the Habsburgs were able to extract huge tax revenues from rural Castile. These taxes, borne principally by peasant-farmers, supplied the bulk of the cost of the Habsburg imperial ventures. But despite the crushing burden of high taxes, Habsburg Castile after the 1521–22 Comunero revolt was remarkably free from political revolts. Nader concludes that the reason for this political docility was that personal liberty was not an issue for Castilians, for they had personal freedom and enjoyed a considerable measure of self-government through their municipal councils.

This work will stand as a landmark in the historiography of early modern Spain. Nowhere else is there so lucid and complete a description of the structure of municipal government in Habsburg Castile, of the rights and obligations of citizenship, of the juridical system, or of the complex relationships between citizen, municipal government, lord, and crown. Nader's prose is a model of clear writing, her scholarship is impeccable, and the book is fascinating reading, thanks to numerous vignettes of rural life in sixteenth- and seventeenth-century Castile. It is imperative reading for specialists in early modern European history and urban history. And Latin Americanists will also find it highly useful because of the enormous implications for Spain's New World colonies.

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DOUGLAS W. FOARD. *The Revolt of the Aesthetes: Ernesto Giménez Caballero and the Origins of Spanish Fascism*. (American University Studies, number 70.) New York: Peter Lang. 1989. Pp. x, 257. \$42.50.

This brief intellectual biography of the representative Spanish man of letters, Ernesto Giménez Caballero (1899–1988), is a valuable contribution to a sparsely cultivated field. As Stanley Payne rightly notes in his introduction, the history of ideas in Spain “has yielded a comparatively modest bibliography” (p. ix) by Spanish and foreign scholars alike. We know a good deal about major figures of this century, but relatively little information exists on lesser writers who often exemplified the climate of ideas at critical moments in Spanish history.

As a member of the Spanish literary generation of 1927, Giménez Caballero entered public life as a passionate advocate of poetic modernism and vanguardism. Influenced by his early mentor José Ortega

y Gasset's *Invertebrate Spain* (1921) and *The Dehumanization of Art* (1925), he was never as apolitical as many of the other young writers who put Madrid on the literary map of Europe in those years. Inspired by the note of political and cultural crisis sounded by the literary generations of 1898 (Miguel de Unamuno, Pío Baroja, Antonio Machado, and others) and 1914 (Ortega, Américo Castro, Manuel Azaña, and others), the young essayist and later editor of *La Gaceta Literaria* (1927–32) seized on literature as a vehicle for reestablishing Spain's importance in world affairs.

Douglas W. Foard meticulously traces the trajectory of his subject's transformation from literary modernist to reactionary ideologue in quest of a “Latin” Europe informed by a blend of Filippo Tommaso Marinetti's futurism and Benito Mussolini's fascism. Although often thin on the broader sociopolitical context, Foard portrays a mind gradually undone by its passion for cultural nationalism, for Giménez Caballero's increasingly eclectic late work foreshadowed the chauvinistic vision of the national soul pressed into service by Francisco Franco's regime after 1939. After his “conversion” to the faith of Italian fascism in 1928, his rhetoric became increasingly strident, culminating in the imperial fantasies of his *Genio de España* (1932), in which he invoked a risen Spain that would embrace both the Roman and the swastika crosses. Driven by what Fritz Stern has called the “politics of cultural despair,” Giménez Caballero rhapsodized about a “resurrected Holy Roman Empire” and “the reconstitution of the empire of Charles V in . . . a union between Nazi Germany, fascist Italy, and a ‘new Spain’” (p. 183).

As the Spanish Civil War drew nearer, Giménez Caballero's work degenerated into gibberish, but Foard skillfully teases out from this “farrago of nonsense” (Gerald Brenan's phrase) the ideological threads of aesthetic antimodernism evident in his subject's sense of revulsion toward liberal democracy, advanced technology, and other features of modern North Atlantic civilization. The author suggests that his study is but a Spanish chapter of an obviously trans-European story, but apart from an allusion to Gabriele D'Annunzio, the book barely touches the comparative issue. It sorely lacks a section that might compare Giménez Caballero's intellectual development to that of authors like W. B. Yeats, Wyndham Lewis, T. S. Eliot, Ezra Pound, Charles Maurras, Maurice Barrès, and the three representative German intellectuals in Stern's classic study. Resistance to the “threat” posed by secular, pluralistic, and democratic values has been a major theme in the biographies of key writers and artists of this century, many of whom turned reactionary in the name of “culture,” art, or religious faith. Traditionalistic regimes in Spain, Portugal, Germany, and Italy seemed, at various times, to offer shelter to these fancies. Such would be the case, Giménez Caballero hoped, with the Spanish Falange after 1939; that movement's rapid marginalization paralleled the increasing ob-

scurity of its most flamboyant theorist during the postwar period. Giménez Caballero took great pride in his appointment as cultural attaché to Paraguay in 1957 and in his later tenure as the Spanish ambassador in Ansunción. At that distance, he could indulge in quixotic dreams of a new transatlantic union of Spanish-speaking peoples, but for those in power at home, as for most of his literary contemporaries, he had long since become a curious "Robinson Crusoe" best left to his insular meditations.

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EDOUARD M. KAYSER. *Gouvernants et gouvernés face aux épidémies dans le Luxembourg au XVIII^e siècle*. (Anciens pays et Assemblées d'Etat, number 91.) Heule, Belgium: UGA. 1990. Pp. 160.

Historians have an abiding interest in epidemiology, and many studies of plague, cholera, smallpox, and yellow fever have appeared in the last two decades. This book on epidemics in eighteenth-century Luxembourg, at the time a duchy under Austrian control, is in this genre.

The principal epidemic diseases in the Low Countries from 1775 to 1800—no archival records exist for the pre-1775 period—were smallpox (which took the greatest toll in lives), dysentery, and typhoid fever. There were four small epidemics of unknown diseases between 1762 and 1779, followed by a major dysentery epidemic in the summer and fall of 1779. Because accurate statistical data are lacking for many locations, Edouard M. Kayser resorts to descriptive accounts to assert that mortality was high. The government did little to manage that epidemic of 1779, relying on cold weather to end it. Because epidemics typically broke out in late summer, the arrival of cold weather in late fall was often a natural check.

A series of dysentery and typhoid fever epidemics occurred between 1780 and 1784. Once again they followed the usual pattern, appearing in late summer and ending with the onset of cold weather. The last ten years of Austrian rule (the French defeated the Austrians in 1795 and established control of the region) saw the appearance of further localized epidemics associated with famine, malnutrition, and war, such as putrid catarrhal fever, grippe (influenza), smallpox, and scarlet fever.

Kayser analyzes epidemics as a disturbance in the natural and social order, which the government had to manage. To reestablish order the traditional constraining measures were used: imposing quarantine, closing borders, controlling foreigners, requiring health certificates, prohibiting imported merchandise, disinfecting mail and other goods, and expelling beggars and the homeless. Other preventive measures included inoculation and removing cemeteries from churchyards and city centers.

Other than inoculation and cemetery removal, there was little sanitary reform in the Low Countries. Although Joseph II instituted medical and hospital reforms in Vienna (following the French example), similar reforms were not effected in the Low Countries. Medical personnel and institutions were inadequate. For example, in 1788 the Luxembourg city hospital had only sixteen beds for a population of 224,000 in the duchy. The one reform that was instituted—and the main concern of Austrian authorities—was the gathering of statistical information, part of the mercantilist program to obtain an accurate accounting of the state's resources.

Kayser concludes that pathological phenomena reflect the sanitary state and socioeconomic conditions of a society and function as a dialectic between government attempts to enact public health measures and the resistance of peasants to medicalization. Kayser illustrates with some wonderful examples the stubbornness and resistance of peasants to medical care, medicaments, or any official aid. Peasants saw public health measures as an intrusion into their personal lives and a constraint on their freedom.

To readers familiar with epidemics and public health elsewhere on the Continent, this slim volume confirms what we already know. The author demonstrates that in the history of epidemics and public health, Luxembourg and the Low Countries were no exception. The behavior of the populace, especially peasants, also followed a similar pattern. This book is a helpful addition to the growing literature in the history of epidemiology; it confirms rather than challenges what we already know about epidemics and public health in eighteenth-century Europe.

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JØRGEN FINK. *Middelstand i klemme? Studier i danske håndværksmestres økonomiske, sociale og organisatoriske udvikling 1895–1920* [Traditional Bourgeois in a Bind? A Study of the Economic, Social, and Organizational Development of Danish Master Craftsmen, 1895–1920]. Summary in German. (Jysk Selskab for Historie, number 46.) Aarhus: Universitetsforlaget i Aarhus. 1988. Pp. 440.

At the turn of the twentieth century, both socialists and conservatives in Denmark, as elsewhere in Europe, believed that the "old middle classes" were being ruined by economic concentration. Mass production and department stores were seen to displace respectively artisans and independent merchants. Jørgen Fink reexamines the evidence for the years 1895–1920, a time of relative prosperity in Denmark, as it pertains to the masters from the eighteen largest crafts. Given the constraints of time and space, he leaves independent retailers for another volume. The book is based on general and industrial censuses as found in the published works of Danmarks Statistiske

Bureau, organizational jubilee books, the secondary literature, and, less often, archival materials. A complex and varied picture emerges from Fink's analysis. There can be no question that artisans and merchants were in a bind, yet many seemed to adapt and prosper.

The first of the book's two sections describes economic developments generally and then analyzes in detail each of the crafts. The Danish economy industrialized comparatively late; not until 1867, for example, does *Beruffsfreiheit* become law. Denmark's relatively dense rural population was based on the success of the switch to production of processed foods by farmers and small-holders joined in producers' cooperatives, and it supported strong local markets centered on provincial towns. Fink suggests that those crafts that remained tied to the local markets and did not compete with the new industrial products generally did well both in number of shops and in income. No national market for bread developed, for example, and bakers, closely tied to local markets, were able to maintain their economic viability. Some shifted from production to retailing or service (usually repair). One popular alternative to traditional blacksmithing, for example, was to sell and repair bicycles. Some crafts, such as butchering, split into industrial producers, in this case for export, and small shops, often retail.

In the second section Fink explores the organizational behavior of craft masters in general and then craft by craft. Such an exploration, he suggests, would indicate the attitudes of masters to economic change. Three geographically predicated patterns are discernable: rural areas, provincial cities, and Copenhagen. The artisans in provincial cities, typically with shops of one to five employees, dominated the organizational patterns. Despite efforts to the contrary, cross-craft organizations or national ones, with some exceptions, were not a success and the regional craft associations prevailed. More masters in Copenhagen joined the Danish Employers Association (DA) because larger shops (those with five to twenty employees) tended to dominate the majority, and Fink suggests that they were more concerned about labor relations. Differences nonetheless remained between industrialists and craftsmen in DA.

Fink's book reads not unlike one of the many by Danmarks Statistik that he used. It includes a great deal of information, often statistical, with 395 tables in the text and two microfiche cards in the back with more statistics. Synthesis and conclusions are briefly and tentatively drawn, and the book is positivist in tone, again, much like the works by Danmarks Statistik. The artisans' and merchants' voices are not heard, except indirectly through the analysis of their organizational behavior and the jubilee books of their organizations. Yet there is a great deal of specific information here, and, like the works of Danmarks

Statistik, this book will undoubtedly be used for its data, and often.

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WILLIAM IAN MILLER. *Bloodtaking and Peacemaking: Feud, Law, and Society in Saga Iceland*. Chicago: University of Chicago Press. 1990. Pp. 407. \$29.95.

This book is a work whose time has come. Originally attracted to social and legal anthropology, William Ian Miller has hitherto contributed important articles and essays during the past fifteen years to the rehistoricization of the Icelandic sagas. He now brings this process to a culmination in his masterful analysis of medieval Icelandic society drawn from close and astute reading of sagas and laws.

Privileged by scholars during the past two centuries, the so-called family sagas have been mined for political and social data about the pagan tenth century, the purported chronological setting for their narratives. Eventually, grave doubts arose over the viability of oral tradition to convey the rich lore of the sagas safely across the illiterate gulf to the thirteenth century, when they were committed to writing. As a consequence, the family sagas became the sole province of literary scholars, whereas historians limited themselves until recently to the allegedly more trustworthy but artistically less satisfying contemporary sagas that chronicle the twelfth and thirteenth centuries.

The current rehistoricization does not imply a return to a pagan perspective, however, but instead enables Miller to read the family sagas largely as evidence of the authors' own society illuminated by the contemporary sagas as well. In addition, the bulky law code *Grágás*, fraught with chronological problems similar to the family sagas and often eschewed by saga scholars, constitutes the third lode for Miller's analysis. When applying these three genres to the High Middle Ages the author is able to reconstruct the inner workings of Icelandic society in far greater detail than possible elsewhere. Miller's subtitle, *Saga Iceland*, bridges the alleged gap between family and contemporary sagas and indicates his preference for narrative sources over normative evidence.

Written in an engaging and self-confident style and enlivened with numerous case studies, the book is intended not only for Old Norse specialists but also for social historians and even the general public weaned on numerous works in the emerging field of law and literature. After a first chapter introducing institutions and people of authority, the second chapter presents the sources. Particularly valuable for Old Norse novices is Miller's translation and close reading of an entire short story, *Þorsteins þátr stangarhöggs*, where he highlights two interests that will dominate the remaining work, the minutiae of family and

household arrangements and the "disputing process." Three following chapters are devoted to the economy, household, and kinship. Recorded only incidentally, these subjects must be teased from the sources with less than overwhelming results for the reader. Of particular interest to economic historians is the author's demonstration of the difficulty with which buying and selling emerged from the traditional forms of exchange, gift giving and raids, governed by the paradigm of balanced reciprocity aimed at maintaining honor.

Feast and feud alike demonstrate this constant and nervous pursuit of honor. In the three concluding and closely linked chapters, Miller applies with perceptual elegance the same model of balance and reciprocity to the activities of feuding, law and the legal process, and peacemaking and arbitration, all problems occupying the center of the saga world. In particular, his analysis of both the complementarity and contrast between the law and saga evidence is incisive. Although Miller supplies for the general reader illustrations sufficient to follow his main arguments, the richness of the evidence here is such that intimate knowledge of the entire narrative and legal corpus is desirable to appreciate fully the densely textured discussion.

It is undoubtedly salutary to reassign to the High Middle Ages those sagas purportedly pagan and the law allegedly imported from Norway in the early tenth century, but the author may share with his readers a lingering nostalgia for paganism. He thus generates chronological terms incompatible to the twelfth and thirteenth centuries, such as "early Iceland" (pp. 45, 113), "Dark Age England" (p. 24), "Germania" (p. 346), and even "early Germania" (p. 89), and finds that Christianity's influence was minimal over the peacemaking process. These problems, however, are unsolvable, and do not detract from this major work that deserves a broad readership.

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SVEN A. NILSSON. *På väg mot militärstaten: Krigsbefälets etablering i den äldre vasatidens Sverige* [Toward a Military State: The Establishment of an Officer Corps in the Sweden of the Early Vasas]. Summary in English. (Opuscula Historica Upsaliensia, number 3.) Uppsala, Sweden: Reklam- och Katalogtryck; distributed by Historiska Institutionen. 1989. Pp. 173. 30KR.

Sven A. Nilsson's work is part of a major research project, "The Armed Forces and Societies in Sixteenth-Century Sweden." It is also based on an earlier contribution to the series by Gunnar Artéus, *Till militärstatens förhistoria: Krig, professionalisering och social förändring under Vasasönnernas regering* (The Coming of the Military State: War, Professionalization and

Social Change in Sweden, 1555–1610 [1986]). Whereas Artéus's work concerned the professionalization of the Swedish military class, Nilsson concentrates on the compensation given to this class and its implications in the development of a "military state" in Sweden during the late sixteenth and early seventeenth centuries.

These studies treat a particularly tumultuous time in Swedish history, when the strong and independent Sweden established by Gustav Vasa dissolved into civil war as his youngest son, Duke Karl, fought his older brother King Johan III, wrested the Swedish crown from Johan's son, Sigismund, then established a regency and became king in 1600. Domestic strife was coupled with foreign adventure when Sweden joined the battle to dominate the Baltic states. Its rivalry with Poland intensified when Sigismund became Poland's king but could not hold his throne in Sweden. Under such circumstances the Swedish army grew in size and its officers had to be rewarded.

Because of the frequent conflict, the number of foreign and non-noble officers in the Swedish army swelled to nearly equal those of native-noble stock in the late sixteenth century. The importance of these first two groups of officers increased particularly during Karl's rule because most noble officers supported Sigismund in the civil war. Land was an important form of compensation to military officers because the state lacked suitable alternatives and, because of its enduring worth, it was the medium most favored by the officers. Native nobles, particularly the upper echelon, were more likely to receive land in Sweden, whereas non-noble and foreign officers received land in Finland and the conquered Baltic provinces. The land grants took many forms, including donations, entailing entire estates with tax-exempt status or, more commonly, with limited rights to the taxes assessed on individual farms or districts. Remuneration might also consist of the revenues associated with administrative offices such as the *häradsövding* (district judge).

Land was given not only for services rendered but also might have had associated with them the condition of future military service. In Finland and the Baltic states, for example, officers often were required to serve in the cavalry and asked to muster additional soldiers. Although contrary to the military changes taking place in the west, this service was needed to counter the similarly armed Poles, who were Sweden's main adversary in the Baltic. So serious was Karl's need for troops that he even required military service of Sweden's nobles after he pardoned them for supporting Sigismund.

Although in one sense appearing to be a re-feudalization of the Swedish military system, this "Swedish model" for compensation, Nilsson contends, was the forerunner of the *indelningsverk*, the policy followed by Gustav II Adolf and his seventeenth-century successors that provided all Swedish military personnel with land in return for military service. In other

words, the *indelningsverk* system began much earlier than has heretofore been realized, and a system originally established for the benefit of military officers became the economic base for a native standing army that was both unique and very effective. With this work Nilsson has therefore added a new dimension to the debate on this subject.

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GER VAN ROON. *Small States in Years of Depression: The Oslo Alliance, 1930–1940*. Assen, Netherlands: Van Gorcum. 1989. Pp. xi, 382. f. 79.50.

The English title of this book suggests that its emphasis is on economic relations and that there was such a thing as an Oslo alliance. Neither is correct. A literal translation of the Dutch title conveys Ger van Roon's emphasis accurately: *Small States in Times of Crisis: From Oslo States to Benelux*. Translation problems also mar the text throughout: Germanic sentence structures, a Germanic obsession with commas, a rich variety of misspellings and typos, and even an occasional double negative all reveal that the translators have a less than native familiarity with the English language.

But those who persevere through the nearly 260 pages of text and 87 pages of footnotes will be amply rewarded, for van Roon's authoritative monograph draws on all relevant sources in nine languages, including archival research in twelve countries. His stated purpose is to enlarge the perspective in which the 1930s are usually viewed by examining relations among small states rather than between small states and great powers, or among great powers alone. That makes the Oslo states a perfect case study, representing as it does an ambitious attempt by a group of small states to achieve economic and political security in a decade of turmoil by mutual reinforcement instead of dependence on great-power patrons.

It did not work, of course. The Oslo Convention of December 22, 1930, signed by Norway, Denmark, Sweden, the Netherlands, and Belgium-Luxembourg and joined two years later by Finland, was meant to stimulate trade among the partners by lowering tariffs. In fact, it never amounted to more than a declaration of intent. Belgian Foreign Minister Paul-Henri Spaak's assessment, expressed in 1938 but always true, was that the group was "neither an alliance, nor an entente." It was merely "a community of aspirations, ideals, and sentiment" (p. 234). But the group certainly never achieved solidarity. Hit hard after 1932 by the impact of Britain's Commonwealth preference system, members erected trade barriers that they also enforced against each other. Soon the partnership effectively broke up into Scandinavian and Benelux regional subgroups, paving the way for post-1945 integrative formations.

Most of the book deals with the political efforts of

the Oslo states to stay out of the rising great-power conflict after 1933 and their reactions to the outbreak of war in 1939, years that saw their "transition from independence to dependence" and demonstrated the "limited opportunities available to smaller states in international politics" (p. 355). It is a sobering tale.

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JAMES J. SHEEHAN. *German History, 1770–1866*. (Oxford History of Modern Europe.) New York: Clarendon Press of Oxford University Press. 1989. Pp. xvii, 969. \$64.00.

James J. Sheehan enjoins his readers to put in abeyance the Prussocentric eschatology that even today informs much of what is written about modern German history. Try to imagine, he proposes, that 1871 was not the ineluctable end toward which German history had been striving since the late eighteenth century.

Proceeding from this premise, Sheehan has produced what is by any account a magisterial exercise in historical reconceptualization. The seemingly arbitrary choice of 1770 as his starting point is felicitous, for it avoids the teleological assumptions that would have been implied had he chosen to begin, say, in 1740 or 1763. Guiding Sheehan's account is a firm belief in the open-endedness of German history. Convinced that the major historical developments of the period "cannot be subsumed within a national narrative whose foreordained destination is Bismarck's Reich" (p. 913), Sheehan stresses the diversity of German social, political, and cultural life.

Accordingly, his starting point is the fragmentary world of the Holy Roman Empire, a notoriously complex subject that Sheehan handles with admirable lucidity and insight. Drawing on the interpretive frameworks of Otto Brunner, James Vann, and Mack Walker, Sheehan emphasizes that the empire cannot be understood in post-Hobbesian terms of state sovereignty. The institutions of the Old Reich served "not to clarify and dominate but rather to order and balance fragmented institutions and multiple loyalties" (p. 14). With the outbreak of the French revolution, the particularistic structure of the empire proved a source of both strength and weakness. Although the empire's mosaic of diverse political cultures proved relatively resistant to revolutionary appeals, that very same fragmentation made its territories all the more vulnerable to external attack. Territorial rivalries, along with the limits that localism and particularism placed on the ability of princely rulers to mobilize their populations for warfare, ensured that the empire would not survive the military challenge of Napoleon.

As in other recent surveys by Thomas Nipperdey (*Deutsche Geschichte 1800–1866* [1983]) and Hans-

Ulrich Wehler (*Deutsche Gesellschaftsgeschichte*, 2 vols. [1987]), Sheehan underscores the modernizing impact of the Napoleonic era. Like Wehler, however, he believes that reform was most efficacious where it accelerated changes already at work in the eighteenth century. In the case of Prussia, Sheehan deems the Stein-Hardenburg era important less for the reforms it produced than for the nationalist mythologies it later engendered (here, as in the first volume of Wehler's *Gesellschaftsgeschichte*, Freiherr von Stein seems to be emerging as one of the most overrated statesmen in German history). It was elsewhere, above all in medium-sized states such as Bavaria and Württemberg, where the reforms of the period proved most lasting. The most important of these came in the administrative sphere, with the result that the fragmented particularism of the empire had by 1815 given way to a political structure dominated by the enlarged and more consolidated states of the German Confederation.

This is familiar enough, but the virtue of Sheehan's account lies in his refusal to see the territorial consolidation that followed in the wake of Napoleon as a historical milepost on the road to unification. Far from facilitating national unity, argues Sheehan, the more consolidated territories of post-Napoleonic Germany were if anything more difficult to integrate into the framework of a German nation-state. Overall, Sheehan finds no necessary connection between German unification and those nineteenth-century forces traditionally associated with "modernization." Railroads and industrial expansion accelerated regional contacts but also accentuated regional specialization; the differences between the industrialized Ruhr and eastern Prussia or southern Bavaria were heightened, not leveled, by the rapid economic growth that set in after the mid-nineteenth century. So, too, in German cultural life: the years between 1770 and 1866 saw the emergence of German as a full-fledged literary language, but the relationship between German literary culture and German national consciousness is one that historians have postulated rather than proven. As Elizabeth Eisenstein has shown, print culture cuts both ways; it can act as a homogenizing force, but it may also serve to express and reinforce regional and confessional differences. In Germany, more so than anywhere else in nineteenth-century Europe, confession remained a divisive force in cultural and political life. Most of the enlarged German states that emerged after 1815 contained both Protestants and Catholics, thereby ending the continuity of territory and confession—*cujus regio, ejus religio*—that had prevailed for more than two centuries. This confessional heterogeneity proved disruptive in the area of church-state relations, and, even more importantly, limited the popular political base on which German liberalism could build.

As in his earlier work, Sheehan tends to focus on the pathology rather than the potential of German

liberalism. What Geoff Eley noted in his critique of Sheehan's *German Liberalism in the Nineteenth Century* (1978) is not altogether absent in this book, namely a tendency to hold German liberalism to standards of ideological consistency and political inclusion more appropriate to the twentieth century than to the nineteenth (*Central European History* 14 [1981], pp. 278–79). But Sheehan also acknowledges that whatever the weaknesses of German liberalism, something akin to a "bourgeois revolution" had indeed taken place in Germany by 1866. He looks beyond the ostensible failures of the revolutions of 1848 to note their achievements, which included a constitution in Prussia and the final emancipation of the peasantry in Austria. The age of the *Bürgertum* had arrived, its presence acknowledged by commentators as ideologically diverse as Robert von Mohl and Wilhelm Riehl. Germany on the eve of unification was palpably different from what it had been a century earlier, and Sheehan describes what made it so. Here, too, the changes he describes are familiar enough—industrialization, urbanization, the growth of bureaucracy, the decline of feudal relations of dependence in the countryside, the expansion of schools and universities, the growing impact of print culture on people's lives, and so forth.

But did these changes, however profound their impact, make national unification a matter of course? Sheehan believes they did not, and stresses instead the enduring significance of regionalism and particularism on the eve of unification. In the sphere of literature, Sheehan makes a convincing case that German literary realism was distinctive precisely because it shunned "national" themes. Rarely does one find in the works of Wilhelm Raabe, Adalbert Stifter, or Gottfried Keller the broad social and historical landscape so prominent in a Charles Dickens or a Honoré de Balzac. "Again and again," writes Sheehan, "the larger world is subordinated to the smaller one" (p. 834)—the public sphere of politics to the private sphere of domesticity, the outer social world to the inner emotional one. When unification came, it owed less to the *longue durée* of German historical development than it did to the confluence of circumstances that in the 1860s led Prussian policy makers to resort to force as a means of imposing a *kleindeutsch* solution to the German question. They could not have done so, of course, had not the social and economic changes described by Sheehan provided the requisite "blood and iron." Or, as Sheehan puts it, "while the developments we have followed in this book did not make a Prussian answer to the German question 'natural' or 'inevitable,' they do help to explain why, once the fighting began, Prussia emerged victorious" (p. 913).

Historians have long been aware of the limits of Prussocentric interpretations, and Sheehan is not the first to plead for an alternative. But he is certainly the first, at least in the world of Anglo-American scholarship, to produce such a consistent and coherent

reinterpretation. Conceptually elegant but never schematic in his presentation, Sheehan has a gift for rendering the abstract concrete with well-chosen quotes and illustrative examples. The scholarly apparatus is useful but not intrusive, and one senses that Sheehan has actually read and digested the works he cites. All in all, one comes away from this book with a deep sense of admiration for a historian who has not been afraid to reshuffle the deck of historical cards long stacked by generations of *kleindeutsch* historians.

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CELIA APPLGATE. *A Nation of Provincials: The German Idea of Heimat*. Berkeley and Los Angeles: University of California Press. 1990. Pp. xi, 273. \$39.95.

This book, according to its subtitle an examination of the German idea of *Heimat* (home or homeland), is, in fact, a case study of the *Heimat* movement in a single, atypical region: the Rhenish Palatinate or Pfalz, a small border province between Germany and France that was assigned to Bavaria after the Napoleonic wars. Celia Applegate has done a first-class job of analyzing the contribution to the development of Pfälzer consciousness of the various *Heimat* associations, from historical societies to hiking clubs, which from the early nineteenth century on defined and redefined what it meant to be a Pfälzer and to be a German.

Applegate's discussion demonstrates that the *Heimat* idea should not be equated with anti-modernism. On the contrary, she depicts "*Heimat*" as a modern cultural-historical construct, part of an emerging bourgeois public discourse about "Germanness." A "new, more malleable kind of localness" (p. 11), it was fully compatible with the development of a united Germany. The *Heimat* movement treasured archaic dress, castles, and artifacts, and sought to preserve regional dialects, but it also took pride in the growth of cities and made full use of modern means of transport and communications. Far from threatening national unity, it sought to root the political nation emotionally by nurturing a sentiment of community attached to a particular place. Diversity was thus a buttress of unity; regional and national loyalties mutually reinforced one another.

Applegate also claims that the *Heimat* movement's nationalism, at least in the Pfalz, was pacific and nonimperialistic for most of its history. Yet two chapters devoted to the Pfälzer response to the rise of National Socialism (chapters 6 and 7) lead to the conclusion that "the integrity of local culture and identity that lay at the heart of the *Heimat* movement was an early and in some sense willing victim of the National Socialist revolution; its forms persisted, but now infused with the rhetoric of racial superiority and the rituals of German power" (p. 198). Nazification meant "reordering . . . the balance between provin-

vincial identity and national consciousness" in favor of the latter (p. 204). Paradoxically, however, in the chaos of defeat, it was localism that triumphed in the guise of the region's Gauleiter, an enthusiastic Nazi who nonetheless sought to defend "his" Pfälzer against the worst excesses of the regime, and allegedly paid for this with his life (p. 266).

Applegate concludes that "*Heimat*" consciousness survived not only Nazism but also the "Americanization" of popular culture in the Federal Republic and the systematic effort over many years to eradicate provincial identities in the communist east. Provincialism and nationalism have remained inextricably linked in the hearts of contemporary Germans.

This book fulfills only indirectly the author's aim of shedding light on the broader phenomenon of national consciousness in modern Europe (p. x). What it does do, most successfully, is tell the story of a hitherto little-regarded cultural and social movement in a fascinating frontier region and set that story firmly in the context of an important German debate about "*Heimat*" and "Nation."

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BRIAN LADD. *Urban Planning and Civic Order in Germany, 1860–1914*. (Harvard Historical Studies.) Cambridge: Harvard University Press. 1990. Pp. xiii, 326. \$37.50.

Modern German urban planning has been the subject of much interdisciplinary debate, but rarely has this literature concentrated on the connections between planning and social history. Brian Ladd's new monograph takes a useful step beyond such limitations by considering the rise of planning in German urban society from 1860 to 1914.

Ladd discusses urban planning in the context of German notions of "civic pride," a project he undertakes by relating urban form, the identity of planners, and the position of planners in German society and politics. He works thematically, beginning with an introductory chapter on broader social and spatial transformations and then moving to chapters on public health, city extension planning, urban aesthetics, the housing question, the move toward comprehensive planning (including zoning, land policy, transit systems, and annexations), and civic pride. He concentrates on Frankfurt/Main, Cologne, and Düsseldorf, but he regularly considers Berlin, Munich, and other cities.

The book is filled with important insights and evidence. Ladd reminds us that "the social history of the urban bourgeoisie in nineteenth-century Germany has not been much studied" (p. 29). He demonstrates that the legacy of the public health movement of the nineteenth century was a belief on the part of urban elites that they had both the power and duty to create a "healthy" city, however that was

defined. He shows that planning emerged from conflict among planners, city governments, political parties, and interest groups at the local level. He stresses the limits of planning when it came to housing the poor or the working classes, arguing that many initiatives failed because of the dynamics of the capitalist real estate market and the cautiousness of the planners, who were convinced that the market, properly managed, was the "natural" basis of the city. He draws attention to changes in "practical aesthetics," suggesting interconnections between opposed urban thinkers such as Camillo Sitte and Hermann Maertens and demonstrating how the study of urban form can be used to highlight social tensions. And he notes that the civic pride of planners—like that of much of the urban bourgeoisie—"was often defensive, a refutation of antiurbanism more than anything that could be described as prourban" (p. 241).

Of course, there are some shortcomings. Although Ladd discusses the contributions of Social Democratic and Catholic Center party representatives to planning debates, one would like to see more detail about popular perceptions of planning. Ladd notes his indebtedness to Marxist and semiological theories, but he eschews discussing them in any thorough or reflexive way. And, although civic pride replaces class interest and social control as the key theme of Ladd's analysis, its relation to the crucial problem of national identity remains unexamined, leaving bourgeois pride about "belonging to a community or the city as whole" (p. 249) unconnected to the deeper tensions of the political culture.

Nonetheless, Ladd's book should be added to a new growing literature on European and German urban history that includes Richard Evans's work on Hamburg, Andrew Lees's comparative discussion of perceptions of the city and his work in progress on urban reform, and others. Ladd does not change the agenda of current urban history (this was not his goal) but does enrich it substantially, and he suggests how much more there is to say not only about the nineteenth-century city but also about the still inadequately studied twentieth-century city that emerged from it.

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PAUL WEINDLING. *Health, Race, and German Politics between National Unification and Nazism, 1870–1945*. (Cambridge History of Medicine.) New York: Cambridge University Press. 1989. Pp. x, 641. \$69.50.

Paul Weindling seeks in this very long book to chart the "transition in science and medicine from emancipatory liberalism to state-oriented expertise" (p. 80) in a Germany united for the first time between 1870 and 1945. Weindling posits a fundamental continuity of aim among German biologists, scientists, and doc-

tors toward "a type of biologically based collectivism" (p. 578), as well as institutional and professional continuities in the same realm extending to, through, and beyond the Nazi era. At the same time, however, Weindling also highlights discontinuities and disagreements among the various individuals and groups turning toward more or less eugenic solutions to combat what from the late nineteenth century onward they saw as the crisis of "social misery of unbridled individualism" (p. 69). A tradition of political authoritarianism, the social, political, and environmental effects of industrialization, and the radicalizing and centralizing effects of the Great War combined with "emergent professionalism" and "bourgeois *Angst*" (p. 81) to create a scientific obsession with degeneration, eugenics, and race. Eugenics itself passed from being "a liberal and secular ideology of social reform" (p. 25) to being a shrill statist demand for collective purity and control. Thus, when the Nazis introduced "the principle of compulsion" (p. 526) into "[b]iologically based politics" (p. 576), they built on powerful extant trends in a Germany that suffered a lack of "radical opponents to eugenics" (p. 579) common in Britain and the United States.

The virtues of Weindling's book include its comprehensiveness, its wealth of archival and bibliographical sources, and its long view of its subject from the 1860s to the 1960s. Weindling makes a solid contribution to the recent literature on the history of science and medicine that emphasizes the social construction and political exploitation and implementation of science and pseudoscience. He also provides an exhaustive documentation of arguments over mortality, morbidity, contraception, abortion, homosexuality, alcoholism, tuberculosis, venereal disease, and other health issues as well as the political activities of the various interest groups contending for the attention and support of a state "that was not an impersonal and detached monolith" (p. 269). And Weindling's conclusion about the dangers of the "aura of expertise, professionalism and technical specialization" (p. 582) engendered by biology and medicine holds an important lesson for society in general.

The limitations of this book include a prose style that blunts the reader's attention with tramping legions of subject-verb-object sentences. And while the strict chronological organization of the book makes sense, it lacks the thematic chapter introductions and conclusions that not only assist the reader in organizing the narrative and its arguments but that also mark an effectively concise text.

As for Weindling's analysis itself, although comprehensive and compelling in terms of the evolution of "racial hygiene" in Germany, there is not enough emphasis on other relevant fields of inquiry into health and illness that not only opposed eugenic thinking but were also influenced by it and attempted to use it to promote their own agendas. This renders Weindling's discussion of Nazi Germany the least satisfying part of the book. Even though the Third

Reich has been the most exhaustively researched era of modern German history, one might have expected this segment of Weindling's study to be as groundbreaking in terms of detail as earlier sections of the book. That this is not the case is due to his justifiable insistence on the Third Reich as being the culmination of the eugenic trend in German biology and medicine. Thus, although Weindling lists both Ulfried Geuter's *Die Professionalisierung der deutschen Psychologie im Nationalsozialismus* (1984) and my own *Psychotherapy in the Third Reich* (1985) in his bibliography, he does not use their arguments in discussing the Nazi emphasis on productivity and efficiency. This is an important omission because the fields of psychology and psychotherapy made professional gains under Nazism as a result of their emphasis on repair and enhancement of "racial comrades" (*Volks-genossen*). As a result, Weindling's discussions of the Nazi persecution of homosexuals and SS projects dealing with infertility leave out consideration of "therapeutic" options advocated and undertaken under National Socialism. Such pragmatic meliorism was the necessary complement to omnipresent Nazi destructiveness and involved not only a challenge to eugenicist psychiatry but also a concession to Nazi compulsion and collectivism.

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JEFFREY ALLAN JOHNSON. *The Kaiser's Chemists: Science and Modernization in Imperial Germany*. Chapel Hill: University of North Carolina Press. 1990. Pp. x, 279. \$39.95.

The jacket illustration of Jeffrey Allan Johnson's book brings to mind the saying that a picture is worth a thousand words. The cover shows a photograph of four chemists in a laboratory, cropped and framed by the dome of a spiked Prussian helmet. The image is suggestive: inside the conservative shell of Imperial Germany resided the brains of modern science, hard at work for traditional military purposes. That message is indeed central to the author's thesis. Johnson contends that the development of natural science in Wilhelmine Germany was a case of "conservative modernization" in which "existing forms are shielded from the impact of innovation, which develops new forms unconnected to the old." The emergence of basic research outside the universities amounted to a typically German technique of managing social change that the author terms a "quarantine on modernity" (pp. 9–10).

This variation on a familiar theme in German history is part of a larger argument concerning the links between modernization, science, and the power of nation-states. In the author's view, modernization is the process of societal transformation caused by the practical uses of science. The dynamism of modern science in turn is a product of tension between the

international community of scientific discourse and its national (and nationalistic) institutionalization. This spurs scientific competition between nations and results in a "struggle to find the best institutional settings for producing knowledge and for transforming that knowledge into power" (p. 9). By couching their appeals for big budgets and new facilities in terms of the national interest, scientists become involved in politics and eventually in war-related research. Thus, the dynamism of rational scientific inquiry becomes tragically intertwined with the irrationalities of competition among nation-states.

Johnson presents these arguments most fully in the first chapter and in the conclusion of his book, the bulk of which is an analysis of the origins, funding, and early history of the Kaiser Wilhelm Society for the Advancement of the Sciences and the chemical research institutes at its core. The study's central characters are the "Kaiser's chemists," the handful of scientists (Emil Fischer, Friedrich Wilhelm Ostwald, Walther Nernst, Ernst Beckmann, Fritz Haber, Richard Willstätter, Otto Hahn) who after 1905 were directly involved with the innovations culminating in the founding of those institutes and who after 1914 participated in "wartime scientific service for their emperor" (pp. 3, 208). It is in these essentially monographic chapters that Johnson builds the foundation for his broader interpretation about scientific modernization and about Germany's conservative variant of that pattern in particular.

Some reviewers might say that Johnson is more convincing on the first point than on the second. The Kaiser's chemists, with their Nobel Prizes, mandarin roots, and connections with industry and government, might appear as the elite of Germany's scientific establishment rather than as the marginalized exponents of "quarantined modernity." True, their new research institutes developed outside of traditional academic structures, there was opposition from other citadels of power and the eventual form of institutional innovation left untouched existing hierarchies and priorities. But one wonders how unique, conservative, and typically German that constellation really was. As with many other arguments about German uniqueness, this question cannot be settled without comparative analysis, which the author does not provide. Others might question whether Johnson's generalizations can stand on the narrow evidentiary base he presents, or whether his concept of "Prussianization" is particularly profound. Such observations, however, should not obscure the fact that this is an ambitious, stimulating, and well-written book.

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MARILYN SHEVIN COETZEE. *The German Army League: Popular Nationalism in Wilhelmine Germany*. New York: Oxford University Press. 1990. Pp. viii, 176. \$35.00.

This thin book (one hundred twenty-three pages of text) studies an organization with an effective life of two-and-one-half years. It begins with accounts of the background to the Army League's foundation and the military legislation of 1912–13 (thirty pages in all) and continues through a treatment of Army League propaganda (sixteen pages) and a somewhat arbitrary case study of the organization in Württemberg (fourteen pages, including four pages of tables). The only substantial discussion is a twenty-eight-page presentation of the Army League's sociology. The account ends with a brief survey of the war and a glance toward Nazism. Not surprisingly, perhaps, given the brevity of the account, Marilyn Shevin Coetzee's main argument seems to be that the League was not very important.

The book's *raison d'être*, expounded in the introduction, is a disagreement with my own *Reshaping the German Right* (1980; rept. 1991), which presented an argument about the politics of radical nationalism between the 1890s and 1908–14. (I apologize for having to declare an interest, but I was presumably sent this book for a reason.) Consequently, it makes sense to indicate the main areas of dispute.

First, my book explicitly confined the nationalist pressure groups' independent significance to an earlier period, which ended in the watershed of 1908–12 (that is, before Coetzee's account begins), when certain conflicts with the government and its party allies were brought to a head. By 1912–14, radical nationalists were being harnessed to a broader right-wing realignment, and the Army League was a direct expression of this new coalescence rather than a continuation of the earlier anti-establishment critique (as revealed, for example, in the pre-1908 history of the Navy League). Accordingly, using a study of the Army League to criticize my argument is rather to miss the point.

Second, I never claimed that radical nationalists came "exclusively from the petty bourgeoisie" (p. 9). Indeed, I pointed out that to a great extent they shared a social world with their moderate governmental opponents and that in any case it was methodologically and theoretically mistaken to explain the forms of radical nationalist politics by reference to a sociologically constructed notion of interest in such a way. To grasp the mainsprings of radical nationalism, one had to look at the specific political experiences of its activists, not at the social backgrounds of its supporters. Thus, when Coetzee argues that radical nationalism was strictly speaking a minority phenomenon, I could not agree more, for this was precisely my own point.

Third, Coetzee characterizes my study of radical nationalist activity as a "top down" approach, which lacks the kind of "localized study required to flesh out these themes" (p. 9). Here I am mystified. We are all victims of our limited surviving sources, but my book actually marshaled a wide array of local evidence to suggest what it meant to become the member of a

nationalist pressure group. By contrast, Coetzee's evidence is rather thin, amounting to a few prominent biographies and a look at the Army League in a single place, namely, Württemberg (a region rather than a locality).

Last, Coetzee has no concept of ideology. My own account tried to take radical nationalist belief seriously by looking at its structure of argument, granting its coherence, and exploring the experiences and contexts that produced it. At its center was a particular discourse of people and nation that requires a fairly sophisticated understanding of ideology to unpick. Coetzee makes no attempt at the latter and instead takes refuge in a few nontheorized metaphors. Popular nationalism resulted from a "distorted vision" (p. 121) and exercised a "hypnotic appeal" (p. 8); it was "a kind of uncontrollable patriotic virus that ravaged the body politic" (p. 10). This was how Marxists used to be accused of regarding ideology. But they, at least, have moved on.

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WALTER HANNOT. *Die Judenfrage in der katholischen Tagespresse Deutschlands und Österreichs 1923–1933*. (Veröffentlichungen der Kommission für Zeitgeschichte, series B, number 51.) Mainz: Grünewald. 1990. Pp. xxxvi, 328. DM 76.

Catholic traditions that taught contempt for Jews and Judaism are better understood than how those traditions may have influenced the masses of ordinary German and Austrian believers before 1933. Walter Hannot's dissertation, written at Bonn University, explores popular Catholic anti-Semitism by analyzing twenty-two newspapers that spoke for political Catholicism during the decade preceding the Nazi seizure of power. His findings will give scant comfort to those who believe that Judeophobia pervaded the Weimar Republic or who draw straight lines from pre-Nazi to Nazi anti-Semitism.

In Germany, the old Catholic suspicions of Jews occasionally surfaced as press reports critical of their alleged secularizing influences in cultural life and their role in the economy. In rare cases such attacks were couched in *völkisch* vocabulary. More significant, however, were denunciations of Nazi racism and acts of violence against the Jews, sometimes even after Hitler was master of Germany. Moreover, newspapers of the Catholic Center and Bavarian People's parties encouraged a degree of tolerance and integration by reporting in positive or neutral ways on local Jewish religious and social affairs and self-defense efforts. Carefully distinguishing between regions of Germany, Hannot not surprisingly finds that Catholic newspapers in Bavaria were somewhat less likely to be friendly to the Jews than were those in north Germany and, especially, the Rhineland.

A skillful combination of quantitative and qualitative analyses enables Hannot to demonstrate that topics relating to Jews, anti-Semitism, and racism were never major concerns in the German journals he examined. Catholics obviously shared the widespread inclination to underestimate the importance of Judeophobia in Nazi ideology. Themselves recent targets of persecution, German Catholics also tended to be preoccupied with their own problems. And yet, Hannot concludes, the failure of anti-Semitism as propagated by Church intellectuals and academics to find any significant place in the popular Catholic press undermines assumptions that Judeophobia formed a bridge between German Catholics and Nazism. Readers may, however, be just as impressed by the persistence in these newspapers of unflattering stereotypes of the Jews that probably made it easier for Catholics to make their peace with Nazism once Weimar institutions collapsed.

Hannot makes his most significant contribution when he illuminates the poorly understood history of anti-Semitism in Austria. Comments in newspapers close to the Christian Social party suggest that Hitler's homeland harbored far more virulent Catholic anti-Semitism than did Germany. Whereas the German journals usually criticized or defended Jews as individuals and distinguished between devout and secular groups within Jewry, the Christian Social party newspapers damned one and all. Moreover, the Austrian newspapers devoted far more attention to the "Jewish problem," viewing it as an aspect of liberal and Marxist exertions to destroy the Christian character of the Austrian state. Hence their attacks on Nazi racism were exceeded by advocacy of practical measures against the Jews and competition with the Hitlerites for the distinction of being more authentically anti-Semitic. Lacking the experience of a *Kulturkampf*, the Catholic majority in Austria showed little of the sensitivity to minority rights that influenced the German church.

In a long penultimate chapter, Hannot observes political Catholicism from the perspective of two Jewish journals, one published by the *Centralverein*, the Jewish self-defense organization, and the other by the German Zionists. The choice of the latter was a mistake, both because it represented the views of a zealous but small minority and because it was little interested in German domestic politics. Hannot would have been better advised to consult the *Israelitisches Familienblatt*, by far the largest newspaper for German Jews during the period. Had he done so, or had he been in better command of the secondary literature, he would have known that this influential journal endorsed the Catholic Center party during Hitler's rise to power. In doing so it expressed the hopes placed by many middle-class Jews in political Catholicism as much as it did anxiety over the collapse of traditional liberal bastions.

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DAVID JABLONSKY. *The Nazi Party in Dissolution: Hitler and the Verbotzeit, 1923–1925*. (Cass Series on Politics and Military Affairs in the Twentieth Century.) Totowa, N.J.: Frank Cass. 1989. Pp. x, 237.

This monograph by David Jablonsky covers the *Verbotzeit*, or the period from November 1923 to February 1925 when the Nazi party was banned in Bavaria after the failure of the Beer Hall putsch. The book explores the effects that the *Verbotzeit* had on the Nazi party leadership and the role of the Nazi party in the larger *völkisch* movement. Jablonsky's account of the *Verbotzeit*, by far the most detailed to date, is based on relevant secondary works as well as a significant number of police and party archives.

The *Verbotzeit*, Jablonsky shows, was a period of dissension among Nazi leaders and confusion as to the party's role in the *völkisch* movement. Nazi leaders not in exile or prison after the Beer Hall putsch often attacked each other over various issues or went their own way. The relationship of Nazi splinter groups to the larger *völkisch* movement was also problematic. The brief merger of the National Socialist German Workers' party (NSDAP) and the German Völkisch Freedom party (DVFP) into the National Socialist Freedom Movement (NSFB), for example, brought bitter opposition, especially from the north German directory of the NSDAP. Turmoil in the Nazi movement simply reinforced the importance of the imprisoned Hitler. By the fall of 1924, Hitler was increasingly seen as the only person who could hold the National Socialist movement together and enable it to survive. When one confused Nazi member asked Rudolf Hess, "Where do you belong now that the movement is splintered?" Hess replied, "To Hitler! To Hitler who stands above it all" (p. 145).

Jablonsky's study is both interesting and frustrating. Its most significant contribution to our understanding of the *Verbotzeit* is its reevaluation of the Bavarian government's repression of the Nazis following their aborted coup. Contrary to the conventional view that the Bavarian government took only half-hearted measures against the banned Nazis, Jablonsky recounts instead an intense police surveillance of Nazi members and supporters. Moreover, Jablonsky also argues, without a great deal of supporting evidence, that the participation of the Nazi splinter groups in the electoral activities of the *völkisch* bloc in 1924 provided the foundation for Hitler's conversion to a legal strategy that emphasized the creation of a mass political party and participation in elections. Otherwise, this account of the *Verbotzeit*, for all of its details, reveals little that we did not already know about the Nazi party's travails during this time period.

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BERNHARD R. KROENER *et al.* *Das Deutsche Reich und der zweite Weltkrieg*. Volume 5. *Organisation und Mobili-*

sierung des deutschen Machtbereichs. Part 1, Kriegsverwaltung, Wirtschaft und personelle Ressourcen 1939–1941. (Militärgeschichtliches Forschungsamt, Beiträge zur Militär- und Kriegsgeschichte.) Stuttgart: Deutsche Verlags-Anstalt. 1988. Pp. xviii, 1061. DM 78.

This immense work is a recent contribution to an ambitious series edited by the historians of the Militärgeschichtliches Forschungsamt (MGFA) in Freiburg im Breisgau and thus represents a milestone in what is surely the largest and most serious investigation currently underway into Nazi Germany and Nazi-occupied Europe during World War II. Hans Umbreit, Rolf-Dieter Müller, and Bernard R. Kroener of the MGFA are the authors of this massive and expensively produced volume. They deal respectively with Reich policy toward occupied Europe, the mobilization of the German economy, and Wehrmacht manpower planning during the first eighteen months of hostilities.

This book is an administrative history much like the many well-known studies written shortly after the war by the Americans and British, the purpose of which was to determine what had gone right, or wrong, in the war effort. Umbreit, Müller, and Kroener have scoured the public archives of the German Federal Republic and produced a book that, in spite of significant shortcomings, constitutes a work of enormous importance.

The authors faced an unenviable task. The time has not yet come when it is possible for Germans to pose in straightforward fashion, without fear of reproach or censure, the fundamental question that must guide a work such as this: How could the Reich in fact have won the war? Technical strategic analyses are therefore all but absent in the thousand pages of text. Instead one is reminded again and again that Hitler was, after all, a criminal idiot whose mania for conquest could have led only to death, destruction, and disaster.

One can easily forget after putting this book down that the Wehrmacht conquered and occupied much of Europe with astonishing ease between September 1939 and January 1942. In their earnest attempt to damn Hitler and his works, the authors provide little insight into either the German strengths or European weaknesses that made this extraordinary, and tragic, advance possible. Here the secondary literature could have been helpful. The authors treat it as of little account. In footnoting they cite from published works only to supplement archival material, and in the text itself limit discussions to debunking, as if trying to rub the slate clean.

One can perhaps sympathize with Umbreit, Müller, and Kroener's rejection of Timothy W. Mason's many scholarly assertions that working-class resistance undercut Hitler's war effort, if only because, after years of painstaking toil, they could not uncover any evidence to sustain it. The aggressiveness of the authors' attack may also stem from a belief that the popularity

of Mason's untenable thesis has blurred a fundamental truth: Hitler could pursue his mad dream only because for most of the war he had the German people, workers included, in his pocket.

Alan Milward's characterization of Nazi Germany as having used a strategy of Blitzkrieg economics also comes in for heavy shelling in these pages. Yet Milward does not, as the authors mistakenly think, really maintain in *The German Economy at War* (1965) that Hitler disposed of a realistic military strategy that included an economic dimension. He is better understood as having provided a timely reminder that economics and politics set serious limits to the kind of war that the Reich was able to wage.

In their zeal to deny any element of rationality to Hitler's war leadership, the contributors to this volume tend to forget that Europe was ripe for the picking and the Germans were plenty tall and strong enough to pluck what they desired. It took little prompting to stimulate collaboration, which cropped up spontaneously in much of German-occupied Europe and persisted in spite of powerful disincentives. And neither the British blockade nor the stupidity of the leadership in Berlin resulted in shortages that impeded the advance of the Wehrmacht or brought real hardships on the home front during the first two years of hostilities.

The three studies contained in this volume represent substantial advances on anything yet published but are not definitive. Umbreit's account of early war occupation policies is the most comprehensive available and should convince any remaining doubters that neither Hitler nor his henchmen developed a strategy to exploit occupied Europe without destroying it. Yet he does not attempt to explain the relative success of Reich occupation policy in 1940 and 1941. Müller's study is the first thorough examination of economic mobilization prior to the appointment of Albert Speer as armaments minister. The author fails, however, to provide criteria by which to measure economic performance, and his account also suffers from the unfounded assumption that administrative chaos and confusion was somehow unique to the Nazi system of government rather than a disagreeable fact of life in all war economies.

Kroener's investigation of Wehrmacht personnel policies presents fresh data and points to a remarkable new conclusion: As early as summer 1941 manpower shortages condemned the Russian campaign to failure. Yet he has trouble seeing the implications of his material. Is it not significant that even a Wehrmacht commanded in the field by potbellied lieutenants and captains who were veterans of World War I managed to roll over both the French and the Russians? And were not the frequent shifts of labor between army and industry that Kroener uncovers something more than evidence of disarray in planning? Does the ease with which such transfers took place between economic sectors not suggest that Ger-

man society had become militarized to the point that swords and plowshares were all but indistinguishable?

The mass of data presented by the authors of this impressive volume—each section is really a book in its own right—confirms that Hitler's sins of commission and omission indeed shaped the wartime policy of the Reich. One shudders to think that a revanchist Germany might have been led by equally determined but smarter and less-corruptible men.

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JOHN A. MARINO. *Pastoral Economics in the Kingdom of Naples*. (Johns Hopkins University Studies in Historical and Political Science; 106th series, number 1.) Baltimore: Johns Hopkins University Press. 1988. Pp. xii, 381. \$38.50.

John A. Marino's book is the first work in English to appear in a long time on the history of Spanish Naples, and that alone would ensure its importance in introducing a neglected subject to an American scholarly audience. But the book has many other accomplishments. It consists of an *Annales*-style total history of the Dogana delle Pecore di Foggia, the royal sheep customhouse founded in the mid-fifteenth century on the model of the Castilian Mesta, that until the Napoleonic time regulated an extensive network of transhumant pastoralism in the northeastern provinces of the Kingdom of Naples. The Dogana managed the conflicting claims to land of those involved in the production and trade of wool and grain and roughly between one and four million sheep would each winter occupy the fertile plains near Foggia. With an overwhelming wealth of information gleaned from rich archival data and countless early modern treatises, Marino reconstructs all aspects of the Dogana, from its conjunctural developments (five phases, with decline in the early sixteenth and mid-seventeenth century interrupting the prosperous periods) to the social differentiation among the shepherds and sheepowners, from the royal government's strategies and goals to the needs and effects of the market for wool and grain, to the ideological and cultural rationales underlying pastoralism both at the elite and popular culture levels.

While Marino's turgid prose and the hardness of his "hard statistics" (p. 8) might deter some readers, that would be regrettable, because his book offers more than a solid picture of an important early modern institution. Rather than engaging in the ritual condemnations of the corruption that plagued the Dogana, like other institutions of its type, or of the government's (and everybody else's) failure to reform a fundamentally static system that did not provide for the maximization of profits, Marino seeks to understand the rationale of all the participants in the complex social, economic, political, and cultural

mechanism that was the Dogana, and by doing so he makes a methodological point that seems applicable to the study of all early modern institutions. Whereas Marino sees Neapolitan history as fully involved in the major phenomena of early modern Europe, from the development of forms of capitalism to the rise of a centralized state, he argues for the need to abandon "our anachronistic evolutionary bias" (p. 4), and rather understand the ambiguities and priorities of all those involved in the Dogana on their own terms.

The sheepowners' association can therefore be seen as easily combining communitarian aspects with the traditions of pastoral individualism. More importantly, Marino recognizes that the royal government's goals were not limited to the maximization of the high revenues that could be obtained from the Dogana. The government sought instead to guarantee both the grain supply for the city of Naples and its rich income from sheep transhumance, to balance, by institutionalizing them, the conflicts between agriculturalists and pastoralists, and between rich and poor sheepowners, to maintain law and order while and by making all social and economic groups to some extent dependent on the government's patronage and decisions. Marino identifies similarly complex rationales behind the actions of all other members of the Dogana system, and this allows him to see how the Dogana and its pastoral economics were an element of the early modern ideal of "good government" (p. 11). Although cyclical in its history and apparently unchanging in its structures, the Dogana therefore was not a mere irrational survival of former times. Its balancing of antagonisms ensured the accommodation and convergence of different economic and social systems (agriculture and pastoralism) in a structure that offered enough stability and profits to everyone involved, while making development "neither inevitable nor desirable" (p. 2). Although the famine of 1764 made most Enlightenment thinkers attack the privileged pastoral system, its continuation into the nineteenth century shows that it satisfied the interests of many. By illuminating the "works and days" (p. 13) of the Dogana, Marino will, one hopes, also have contributed a reassessment and a more profound understanding of the so-called backwardness of Naples and other neglected provinces of early modern Europe.

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FREDERICK KELLOGG. *A History of Romanian Historical Writing*. Bakersfield, Calif.: Charles Schlacks, Jr. 1990. Pp. viii, 132. \$15.50.

The aim of this study is "to survey the landmarks in historical learning from the birth of Romanian writing . . . down to the present" (p. vi) in six concise chapters. Frederick Kellogg begins with an overview of "early historical writing in the Romanian lands"

and discusses particularly Dimitrie Cantemir, the Moldovan chroniclers, and the Transylvanian school. This work is seen as primarily annalistic, stressing the providential and highlighting the discovery of Romanian Latinity.

The second chapter deals with "modern Romanian historical writing," which began in the early nineteenth century and continued through World War II. Prior to 1918, the principal contributions were in document collection, archaeology, and the further exploration of national origins. The interwar era not only expanded along these lines but also saw the appearance of major professional schools and journals, new in-depth syntheses of the Romanian past, and the maturation of historical scholarship in every domain.

The final survey chapter covers "contemporary Romanian historical writing" since 1944, when Marxism-Leninism was established as the official ideology of Romanian scholarship. Kellogg reviews work done in this era on document publication, efforts to reinterpret the Romanian past in accordance with a somewhat fickle orthodoxy, and the recent outpouring of monographs (although a number of significant names are missing here, such as Paul Cernovodeanu, Șerban Papacostea, Apostol Stan, Anastasie Iordache, and Ion Stanciu, as well as the contributions of Al. Zub, Pompiliu Teodor, and Lucian Boia to the history of history in Romania). His restrained treatment of this now-closed chapter in Romanian historiography, whose net effect was to impoverish and distort an entire culture, is kinder than it ought to be.

Chapter 4 scrutinizes "foreign views of Romanian history" in the nineteenth and twentieth centuries. The works of Hungarian, German, Turkish, and other non-Romanian scholars are set forth, and their major themes and interests are identified. There are some surprising omissions here as well (for example, Glenn E. Torrey, Katherine Verdery, Catherine Durandin, Mary Ellen Fischer, Michael Shafir, Aurel Braun, and Walter M. Bacon, Jr.), the most striking of which is Paul A. Hiemstra's study, *Alexandru D. Xenopol and the Development of Romanian Historiography* (1987). Some lacunae could have been alleviated by Andrea Deletant and Dennis Deletant's *Romania* (1985), which is missing from the bibliography.

The last two chapters briefly analyze the "resources and organization of Romanian historical research" and "current needs of Romanian historiography." For those wishing to start or to pursue further study of the Romanian past, this is perhaps the most useful section of the book, although some of it has been made obsolete by the events of December 1989. There are also two appendixes (a chronology and a map), a bibliography, and a name index (which should be more comprehensive).

Kellogg is successful, on the whole, in carrying out his aim. This is a competent, informative, and reliable survey of Romanian historical writing. The style is lucid, the presentation is succinct without degenerat-

ing into mere catalog, and the richness of bibliographic detail reflects the author's three decades of effort in Romanian studies. This book, the first such treatment to appear in English, will provide a useful introduction to the topic for years to come.

PAUL E. MICHELSON
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M. J. ROSMAN. *The Lords' Jews: Magnate-Jewish Relations in the Polish-Lithuanian Commonwealth during the Eighteenth Century*. (Harvard Judaic Texts and Studies, number 7.) Cambridge: Center for Jewish Studies, Harvard University and the Harvard Ukrainian Research Institute; distributed by Harvard University Press. 1990. Pp. xv, 256. \$21.00.

Although early modern Poland-Lithuania was not, as people then were wont to say, "a paradise for Jews," it was the home of the majority of the Jewish people. In the years between 1500 and 1800, Jews in the Polish-Lithuanian Commonwealth created ramified and complex autonomous institutions, participated significantly in most branches of the economy, and displayed enormous cultural vitality and creativity. By the end of the period they comprised about a tenth of the total population and half of the urban residents of Poland. M. J. Rosman's indispensable book is a thoughtful and thorough examination of the most important constituent among the conditions that made all this possible, namely, the relationship between Jews and the great magnate aristocrats.

On the basis of a rich collection of archival materials preserved in the Czartoryski Library in Kraków, this monograph presents a case study of magnate-Jewish relations by analyzing the situation of the thirty thousand Jews living in some of the thirty towns in the Sieniawski-Czartoryski holdings in the eighteenth century. Little attention here is paid to village Jews. Jewish communities numbering in excess of one thousand existed in twenty of the towns by 1765. In sum, roughly 4 percent of the Jewish population of the Commonwealth lived in the Sieniawski-Czartoryski holdings, mainly in the southern and southeastern parts of the country including Międzybóż (Medzhybizh), and one large Belorussian town, Szklów (Shkłoŭ).

There are seven chapters; the first three provide introductory background on the magnates in general, the Sieniawskis and Czartoryskis, and Jews in Poland. These are followed by two fascinating but perhaps excessively detailed chapters devoted to the economic significance of Jews as merchants and as "arendators" (lessees of certain monopolies or properties, most commonly the right to produce and sell alcoholic beverages). The sixth chapter describes the careers of two unusual figures: Moses Fortis, physician and factor to Elizabeth Sieniawska, and Israel Rubinowicz, general manager (*ekonom*) of the Rytwiany-Lubnice complex. Rosman emphasizes that as wealthy and

powerful as these men were, they were not comparable to the court Jews of Central Europe: rather than helping "to catalyze a new economic and political system," they "were fulfilling a subordinate role within Polish feudalism" (p. 184). The seventh chapter suggests that the tendency of the magnates to consolidate their administrative hold significantly weakened Jewish communal autonomy. There is an excellent glossary, but the index could have been more detailed.

This monograph is of essential importance to historians of Poland and of the Jews. Rosman shows that magnates were normally benevolent toward their Jews because Jewish economic interests generally complemented their own. Moreover, in stark contrast to the usual picture in Polish historiography, Rosman demonstrates convincingly that Jews played a central role in the economy of the estates of the aristocrats. As a consequence of their role, Jews were drawn, more than is usually noted in Jewish historiography, into the main arena of society; social, cultural, and political associations with Christians "were unavoidable" (p. 209). Finally, Jews living on the latifundia of the magnates lived with "a large measure of confidence and security" (p. 208).

It is no fault of Rosman's that he has had to fill a lacuna in Polish historiography, but it ought to be embarrassing to historians in Poland that a foreigner has written such an important chapter in the history of their country.

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HILLEL GOLDBERG. *Between Berlin and Slobodka: Jewish Transition Figures from Eastern Europe*. Hoboken, N.J.: Ktav. 1989. Pp. xvii, 269. \$25.00.

Hillel Goldberg's book is a series of portraits of Jewish intellectuals who were raised in the traditionalist milieu of tsarist Russia and settled in twentieth-century Germany, America, or Israel. The author's intent is to lay bare the emotional and ideological conflicts in adapting Jewishness to new cultural circumstances by men who came to reflect, each in his own way, influential modes of twentieth-century Jewish self-understanding.

All of Goldberg's subjects studied at the Slobodka rabbinical academy (on the outskirts of Kovno, Lithuania), which flourished from the early 1890s until its destruction in 1941. The Slobodka *yeshivah* was a leading center of rigorous talmudic training; it was also a home of the *Musar* (ethicist) movement of inward, spiritual reform within East European non-Hasidic traditionalism in the nineteenth and twentieth centuries. In Goldberg's typology, Slobodka is one pole and Berlin is the other of the common experience of these men, who all studied at the University of Berlin in the 1920s. Berlin comes to represent the sustained effort to absorb modern philosophy, litera-

ture, and historiography into Judaism—the synthesizing of the Jewish tradition with critical *Wissenschaft*, predominant in the West since the Enlightenment.

Despite this theoretical focus, the material is sometimes tangential to the main theme. The first chapter discusses Rabbi Israel Salanter, the father of the *Musar* movement, who lived before the founding of the Slobodka *yeshivah* and did not settle permanently in Western Europe. Succeeding chapters deal with Harry Austryn Wolfson, the eminent Harvard historian of medieval philosophical traditions, who is described in unflattering terms as a man fleeing from Jewishness and ordinary humaneness; Isaac Hutner, head of the Brooklyn *yeshivah* Mesivta Chaim Berlin, a center of the renewal of strict orthodoxy in America; Joseph Baer Soloveitchik, who became the pre-eminent talmudic authority of Yeshiva University in New York and philosopher of the existentialist "halachic man" who lives by Jewish law as orthodoxy defines it; Abraham Joshua Heschel, one of the most evocative religious writers of America during his tenure at the Jewish Theological Seminary in New York. The final chapter considers the little-known Joseph Zev Lipovitz, who managed a *pension* near Tel Aviv and maintained a connection with a *yeshivah* in nearby Benei Berak, a main center of Orthodoxy in Israel.

This book is an engaging, opinionated presentation of a handful of men who, in one way or another, had considerable personal impact on the author. The book's usefulness lies in its concern for ambivalences of a different sort than those usually treated by scholars who concentrate on the recognized panoply of acculturated Jewish philosophers, intellectuals, and social radicals; instead, this handful of Jewishly erudite intellectuals acquired some degree of worldly sophistication and took up the defense of Jewish piety and religiosity (except for Wolfson) at a time when it was not fashionable to do so. The most apologetic treatment is given to Rabbi Hutner, an intimidating, authoritarian, closed-minded man who came to be quite hostile to modern secular learning, perhaps indicating that Goldberg is not sanguine about an "authentic" Judaism fully based on critical Western scholarship. Judgmental and subjective, his book does deal with a neglected chapter in the recent history of Judaism and calls attention to certain inherent tensions in synthesizing dual cultural identities that many cultured religious intellectuals have experienced.

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ADAM WANDRUSZKA and PETER URBANITSCH, editors. *Die Habsburgermonarchie 1848–1918*. Volume 6, part 1, *Die Habsburgermonarchie im System der internationalen Beziehungen*. Vienna: Österreichische Akademie der Wissenschaften. 1989. Pp. xvi, 819. S 770.

This volume is part of the impressive series on the history of the Habsburg empire from 1848 to 1918 edited by Adam Wandruszka and Peter Urbanitsch under the sponsorship of the Commission for the History of the Austro-Hungarian Monarchy of the Austrian Academy of Sciences. Other books have dealt with economics, administration and law, the nationalities, religion, and the military. This volume represents the first of a projected two-part publication. The second part, to appear later, will focus on the attitude of the other European powers toward the monarchy.

Because of the difficulty of covering a book with eight major divisions and fourteen authors, this review will first present a short summary of the contents of the book and then concentrate on the central major chapter on diplomatic history. The first chapter (by Helmut Rumpier) studies the origin and development of the Habsburg foreign ministry and the position of the minister; the second (by Heinrich Pfusterschmid-Hardtenstein) covers the education of Habsburg diplomats at the Oriental and Consular academies. Two later chapters (Leopold Kammerhofer, Günther Ramhardtter) discuss press policy and propaganda respectively. The economic and commercial aspects of foreign policy are considered in six sections, written by different authors (Klaus Koch, Lothar Höbelt, Emil Palotás, Ugo Cova, Hermann Hagspiel, and Eduard G. Staudinger and Siegfried Beer), who analyze Habsburg relations with the Zollverein, Germany after unification, Russia and the Balkans, Italy, France, and Great Britain. Thomas Kletečka writes an excellent chapter on the parties and groups in Cisleithania, including the attitudes of the Germans, Czechs, Poles, Ruthenians, Slovenes, the army, and the Social Democrats. There is no similar study of the national parties of Transleithania. A chapter by István Diószegi on the political and diplomatic interests of Hungary concerns only the Hungarian section of the population. The failure to discuss the South Slav and Romanian parties is especially to be regretted because their position had such a strong influence on the decisions of the Habsburg leadership in July 1914.

Although most of the authors are Austrian, the central article on Austria-Hungary among the great powers is by the British historian Francis Roy Bridge. The obvious choice for the subject, he is the author of standard works and articles on Habsburg foreign policy. His conclusions result from a thorough familiarity with the Habsburg archives and with the literature on the subject.

In general, Bridge's survey reflects the pessimistic tone found throughout the book. In fact, the first line of the introduction by the editor of the series, Wandruszka, quotes Franz Joseph in the last years of his life: "I have known for decades how much we are an anomaly in the present-day world" (p. xi). Bridge in the same spirit traces the steady decay of the international position of what he correctly assesses as the

weakest of the great powers. With an army that was consistently starved of revenues and that occupied a central geographic position, the monarchy needed the support of at least another great power or at least the recognition that the state was a European necessity. After 1815, Russia and Great Britain provided this backing but abandoned their position as a consequence of Habsburg policy during the Crimean War. The alliance with Germany after 1871 did not provide the monarchy with a consistently reliable ally or a firm supporter of Habsburg interests. During the course of World War I, Germany reduced the monarchy to the position of "a helpless, if also unwilling satellite" (p. 341).

In his examination of the successive diplomatic crises, Bridge covers first the revolutions of 1848, then the Crimean War, and finally the unification of Italy and Germany. After 1871, the emphasis falls naturally on the Eastern Question. Excluded from participation in the great age of overseas European imperial expansion, the Habsburg empire could only hope to extend its influence southward into the Balkans, but such a policy was not advisable because its interests required the maintenance of the status quo. The Habsburg leaders regarded the Ottoman empire as the best neighbor in the region and they feared the effects of the Balkan national movements on their internal affairs. Convinced that only Russia would benefit from the exclusion of Ottoman rule from the Balkans, they were deeply influenced by their exaggerated fears of Russian aggressive goals. The one Habsburg advance, the occupation and subsequent annexation of Bosnia-Herzegovina, is discussed in detail. The author explains that the Habsburg acquisition of the province was not part of an expansionist policy but an attempt to draw a permanent frontier with the Ottoman empire. He quite correctly discounts the idea of a "march to Saloniki" (p. 318). It was, of course, Balkan events that drew the monarchy and all of Europe into World War I with disastrous results for the monarchy. The final defeat and dissolution, the author concludes, resulted when "the ruling elite decided not to follow further a compromise peace joined with federal reforms, but to set all on a German victory" (p. 372).

This volume presents a detailed, balanced, and thorough examination of Habsburg foreign policy. The choice of subjects and the expertise of the authors gives the reader a wide perspective on the crucial events of this dangerous period. The book is obviously essential reading for specialists in European international relations and in Habsburg history.

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IVO BANAC. *With Stalin against Tito: Cominformist Splits in Yugoslav Communism*. Ithaca: Cornell University Press. 1988. Pp. xvi, 294. \$32.50.

As Yugoslav communism disintegrates and that country descends day-by-day deeper into anarchy and civil war, one must make an effort of will to recall how, until recently, many Western academics looked to Yugoslavia as a viable model of communism, less authoritarian and more humane than the Soviet version. As Ivo Banac points out, observers of Yugoslavia have often been too inclined to assume that Belgrade's resistance to Stalinism and Soviet domination was "necessarily a sign of emerging political pluralism" (p. 257). In fact, however, the Yugoslav Communist party (KPJ) did not become more tolerant immediately following the rancorous split with Joseph Stalin in 1948; rather, it devised its own version of "home-grown Stalinism" (p. 136). Tito's "conflict with Stalin played the same part in the shaping of Yugoslavia's political system that collectivization and the purges of the 1930s played in the history of Soviet communism" (p. 257).

In this interesting and important monograph, Banac focuses on the losers in this struggle, those Yugoslav communists who sided with the USSR against Tito's rule. Long portrayed as traitors and opportunists by official Yugoslav histories, these *ibeovci* (so-called after the initials of the Soviet-dominated Informburo to which they remained loyal) were a diverse and contradictory group. Some, especially in Serbia, sided with Stalin because they believed that the USSR would support greater Serbian dominance within Yugoslavia; others hoped that Moscow would weaken Belgrade's increasingly centralized power; still others, knowing little about the Soviet Union other than what they read from Stalinist propaganda, contrasted the pristine Soviet communism of their imaginations with the squalid reality of "real existing socialism" in their own country. In short, support among Yugoslav communists for the Soviet Union in 1948 reflected the diversity of domestic discontent with the new communist order.

The first chapters of the book trace the history of factional fights within the KPJ both before and during World War II, showing how the fights "all concerned nationality relations" (p. 115). Particularly important in shaping the postwar Yugoslav-Soviet clash was the legacy of tension between Moscow and Tito's partisans. As has long been known from Milovan Djilas and other historians, Stalin cautioned Tito to play down his communist radicalism during the war so as not to alarm the Western allies. Here, Banac engages in a little unconvincing Kremlinology, suggesting that Stalin was a "rightist," not really interested in imposing communist regimes in Eastern Europe at the expense of a confrontation with the West; he argues that Andrei Aleksandrovich Zhdanov led a "leftist" faction, urging greater communist assertion abroad and more "intellectual freedom" in the USSR (p. 25).

In support of this contention, Banac cites Stalin's suggestion to Tito that he should accept the return of the Yugoslav king: "You don't have to return him

forever," Stalin said, "Only for a while, and then slip a knife into his back at the opportune moment" (p. 14). This comment does not, however, prove that Stalin and Tito differed over ultimate aims. For Stalin, the reality of power was more important than symbols of specific programs; his dispute with Tito was not one of principle, as Banac seems to believe, but rather of tempo and tactics.

Even so, any insubordination by a lesser communist state, even over tactics, was intolerable to Stalin, who made clear his disdain for partisan martial achievements. This insult to Yugoslav communist self-esteem was Stalin's "most glaring error" (p. 127) in Banac's view, because it bound the otherwise fractious Yugoslav communists together and helped to atomize domestic opposition to Tito. As the Moscow-Belgrade split widened, the Soviets claimed that Belgrade could not create socialism without assistance from fraternal communist states, an argument that, as Banac rightly points out, negates Stalin's earlier program of "socialism in one country." Stalin's bullying drove most Yugoslavs together and enabled the ruling elite to brand opponents not only as factionalists but also as traitors.

The challenge of the *ibeovci* was doomed to failure because they were only "an elite problem" (p. 151), with no large social base; and they "did not really constitute a movement, [because] they had no single leadership and no consistent program" (p. 256). Although ultimately unsuccessful, they were not insignificant: the *ibeovci* may have constituted as much as one-fifth of the KPJ and infected the officer corps in "epidemic proportions" (p. 159). The motives of the *ibeovci* tell us a great deal about the tensions in postwar Yugoslavia; the reasons for, and manner of, their defeat tell us even more. We are indebted to Banac for a fine portrait of this important and neglected group.

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JUDITH PALLOT and DENIS J. B. SHAW. *Landscape and Settlement in Romanov Russia, 1613-1917*. New York: Clarendon Press of Oxford University Press. 1990. Pp. xiii, 318.

Judith Pallot and Denis J. B. Shaw have renewed their collaborative efforts, shifting their attention from the Soviet Union to European Russia during the three centuries of Romanov rule. Their purpose as historical geographers is to demonstrate how geographical and historical questions can stimulate these respective disciplines as well as illustrate the diversity of land settlements and human ecotypes in Romanov Russia.

The book is a series of essays divided between the two authors rather than an integrated whole. This is a result of the authors' decision to concentrate on their individual research interests. A drawback of this approach is the unevenness of the articles in terms of

period and subject matter. The five chapters written by Pallot deal with rural Russia after emancipation. Shaw has the more formidable task of grappling with the remaining two and a half centuries. He focuses on the southern frontier and its settlers before turning his attention to trade in the late eighteenth and early nineteenth centuries and finally town development in the Romanov era. The peasant and serf communities of preemancipation Russia are not addressed.

Both authors test traditional theories and debates with differing degrees of success. Shaw's reexamination of the Frederick Jackson Turner thesis in relation to Russia's seventeenth-century southern frontier demonstrates how the centralized state and its military institutions organized the settlement of the frontier. Rather than concentrate on the familiar Cossacks, Shaw looks at the frontier's official military defenders, the *odnodvortsy*. Even though they became almost indistinguishable from state peasants by the end of the eighteenth century, their settlements remained distinctive, exhibiting some of the individualism that characterized frontier pioneers. Shaw also does an admirable job in challenging the Soviet scholars' claim that Russia had developed a national market before the railway. By examining regional differences, he argues that an all-Russian market was only in the process of formation. It had not yet enveloped "the periphery or even all the many interstices within the network of waterways" (p. 215). Pallot's study of peasant domestic industries in the post-1861 Moscow province challenges the Marxist-Leninist premise that peasants were in danger of losing their class distinctiveness. She paints a picture of an adaptive peasantry able to resist capitalism's corrosive forces. Pallot is less successful in discussing theories of regionalization vis-à-vis the late nineteenth-century Russian peasantry. She presents the respective arguments of A. N. Chelintsev and V. K. Yatsunskii, however, without subjecting them to rigorous analysis.

Through a series of case studies Pallot and Shaw argue that historians should pay more attention to the regional diversity of Russia. It should be noted that local studies are increasingly pervading the work of social historians on the pre and postemancipation Russian peasantry. Pallot and Shaw's microanalyses sometimes fall short of the interdisciplinary approach of social history. On the one hand, Shaw's study of Voronezh province and Pallot's examination of peasant ecotypes in Archangel and Kharkov province are purely descriptive. On the other hand, Pallot's discussions of the differing responses and reactions of late nineteenth-century Mennonite and peasant communities in Samara province to the Stolypin reform in two similar districts of European Russia are superb. In these cases environment was not totally determinant of the choices communities made regarding their agricultural practices and land use. Culture and material resources were important in shaping Mennonite practices, and the wide scope of possibilities

contained in the Stolypin land reform produced dissimilar peasant responses.

Despite its unevenness and analytical shortcomings, this book deserves the attention of both historians and geographers. It contains a wealth of information and excellent maps of which historians should take note.

CHRISTINE D. WOROBEC
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W. BRUCE LINCOLN. *The Great Reforms: Autocracy, Bureaucracy, and the Politics of Change in Imperial Russia*. DeKalb, Ill.: Northern Illinois University Press. 1990. Pp. xxi, 281. Cloth \$29.00, paper \$12.00.

W. Bruce Lincoln's latest book is intended to provide the "long-needed synthesis of the Great Reform era" (p. xvi) of the third quarter of the nineteenth century and to place this period in the context of the history of imperial Russia. The author draws on an impressive body of recent American and Soviet scholarship as well as on his own publications and intimate familiarity with archival sources to produce a well-written, concise, and yet wide-ranging account of the era's major legislative acts (the emancipation of the serfs, zemstvo and municipal reform of local government, the new judicial system, censorship regulations, and the military reforms) and their roots in the reign of Nicholas I and the intellectual and political ferment produced by the debacle of the Crimean War. The author also analyzes the impact of the reforms by examining how they and their underlying principles "were tested from the Left and the Right" (p. 163) in the period from 1874 to the revolution of 1905. Lincoln's main and historiographically traditional theme is that the Russian autocracy failed both to modernize the imperial polity and to bridge the gap between the state and society, which the Great Reforms were designed to overcome by instituting the foundations of a civic society in the empire.

Lincoln's treatment of the subject is an appealing but not entirely successful amalgam of scholarly monograph, textbook coverage, and interpretive essay that attempts to place the reforms in their broader political, social, and intellectual context. Lincoln writes well and covers a variety of topics in nineteenth- and early twentieth-century Russian history, and he is particularly skillful in culling illuminating quotations by contemporaries from both archival sources and secondary works. His focus, however, is on the legislative history of the reforms and the bureaucratic politics that engendered them. This is the most valuable part of the book, meshing his own pioneering research on the mentality of the imperial bureaucracy with the findings of specialized and more extensive treatments of individual reforms. The emphasis on the process, however, deserves to be balanced by a fuller treatment of the content and provisions of various reform acts. In turn, this would

allow for more systematic analysis of the conservative redefinition of the Great Reforms after 1874. Such an approach would make this volume more attractive both to comparativists and to Russian and Soviet specialists who might want to use it for teaching purposes.

Lincoln's book, however, is also an interpretation of the political cultures of late imperial Russia. The reforming bureaucrats as well as moderates and revolutionaries within Russian society operated with a set of concepts and terms such as *glasnost'* (public debate of political and social issues and policy options), *zakonnost'* (lawful rule and functioning of government and the bureaucracy), and *proizvol* (autocratic arbitrariness), which defined their agenda and aspirations for the future evolution of Russian state and society. In particular, this political vocabulary circumscribed the limits and content of intrabureaucratic struggles over the course of state policy throughout this period. Lincoln is acutely aware that such generalized and often nebulous ideas embodied a range of denotations and connotations depending on the context and the speaker, and he makes some provocative observations on how their meaning varied over time (pp. 189–91). Unfortunately, given the centrality of this terminology to his conceptualization and argument, Lincoln often takes these ideas for granted and fails to analyze them to the extent and depth they richly deserve.

THEODORE TARANOVSKI
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CHARLES A. RUUD. *Russian Entrepreneur: Publisher Ivan Sytin of Moscow, 1851–1934*. Buffalo: McGill-Queen's University Press. 1990. Pp. x, 270. \$32.95.

In an essay for the collection *Entrepreneurship in Imperial Russia and the Soviet Union* published almost ten years ago, William Blackwell observed that there was "practically no biographical study of the individual entrepreneur, Russian and non-Russian, in capitalist Russia. To the extent that sources are available . . . perhaps the greatest need in the field of Russian entrepreneurial history is to probe inside the mind, family life, and social and cultural experience of the individual entrepreneur." Since then Russian historians have not exactly rushed to meet this need. Business elites are everywhere a reticent breed (although Russian entrepreneurs seem particularly so), and too often sources are not available to probe their minds and family lives. Because individual entrepreneurs are not regarded as major agents of change in Russia, their social and cultural experiences are known to only a handful of specialists. Charles A. Ruud's biography of the Moscow publishing magnate Ivan Sytin is a valuable study of an entrepreneur and his efforts to modernize and democratize Russian society.

Sytin's rise follows the paradigm of the entrepre-

neur in the Western world, and Ruud records his life vividly and crisply. Of humble provincial origins (though, as a portrait of his parents reveals, hardly from "rags"), young Sytin learned the techniques of buying and selling while working for his uncle at the annual trade fair at Nizhii Novgorod. He became a quick and attentive errand boy for a fur trader who printed and sold simple pictures for peasants on the side. His benevolent master helped him get started in the printing business. He "acquired a wife," in Ruud's perhaps all too appropriate words, whose dowry helped him buy his first printing press. By the early twentieth century, Sytin had become the largest publisher in Russia and his newspaper, *Russkoe slovo*, had the largest circulation. Throughout his career Sytin continually expanded and diversified his operations, employed the latest technology to provide a superior product at low cost, recruited able writers and editors, and took advantage of every opportunity to increase his market shares.

Sytin's life illustrates not only the culture of business but also the business of culture in late imperial Russia, and his eagerness to increase his market shares brought on charges of unfair business practices and opportunism. His publishing venture, *Posrednik* (Mediator), in which Sytin printed and distributed books selected by Tolstoy, and the liberal stand of his newspaper displeased the government. At the same time, he made money in the commercial market disdained by the intelligentsia, printed pictures of the royal family, and in wartime published patriotic tracts. For criticizing the inability of the Provisional Government to stop Bolshevik propaganda in the army in 1917, Sytin ran afoul of local soviets, which harassed, censored, and confiscated papers not to their liking, a form of Bolshevism from below. Although Sytin's company was nationalized, Lenin recruited the aging publisher to carry out missions for the new government. As Ruud aptly points out, the New Economic Policy (NEP) did not bring the same relaxation of government controls in publishing as in other small businesses. Sytin could be a Russian broker but no longer a Russian entrepreneur.

Several questions linger, and one wishes that Ruud had explored more the broader implications of the life of one entrepreneur. Given the precarious social basis of Russian liberalism, one would like to know more about the origins of Sytin's liberal views. The biography suggests several—the influence of Tolstoy, Chekhov, and other intellectuals; the oppositional tendencies of independent publishing and news gathering under autocracy; Sytin's own frustrations in dealing with the bureaucracy and censorship; the legacy of populism—but they are not systematically examined. Perhaps when packaged well, national-liberal views "sold," and Sytin the profit-maximizer was willing to sponsor the writers who held them—or to blend, as Ruud puts it, "self-interest and altruism." At the same time, Sytin's was a nonparty liberalism,

and, unlike some prominent Moscow businessmen, he played no direct part in political affairs in the post-1905 period. In any event Ruud's study suggests that there may have been more support for a nonsectarian liberalism among the business class of humble origins than commonly thought.

In writing a biography, Ruud avoids situating Sytin in the "merchant estate," "bourgeoisie," or "middle class." His choice of category is "entrepreneur." However appropriate for understanding the life of one man, this approach makes one wonder what Sytin's life tells us about social identities. Sytin's blend of self-interest and altruism, as well as his other manifestations of the spirit of enterprise, are, despite the cultural differences, strikingly similar to entrepreneurial traits in the West. Ruud's presentation of the entrepreneurial model is, if anything, too brief, and one wishes for more discussion of its implications for the Russian context. Was Sytin's business acumen more prevalent in nineteenth-century Russia than is commonly thought or was it still exceptional? The relationship between entrepreneur and government is important; Sytin clearly enjoyed government cooperation as well as harassment, although the source of protection in higher places is never made clear. Despite the similarities, in one important way Sytin deviated from the Horatio Alger myth. American Horatio Algers grew up supporting the system that gave them their opportunities. Sytin supported writers who challenged the autocracy; as Ruud puts it, he was an "agent of democratization." Despite these flaws, Ruud's biography of Sytin is a most welcome portrait of a very important breed, the Russian entrepreneur.

JOSEPH BRADLEY
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DANIEL P. TODES. *Darwin without Malthus: The Struggle for Existence in Russian Evolutionary Thought*. (Monographs on the History and Philosophy of Biology.) New York: Oxford University Press. 1989. Pp. 221. \$45.00.

Charles Darwin died in 1882. At the Seventh Congress of Russian Naturalists and Physicians, which met in Odessa in 1883, Aleksandr Kovalevskii, an early pioneer in evolutionary embryology, was given the honor of delivering an *éloge* to the great naturalist. Kovalevskii, who received high praise in Darwin's *Descent of Man and Selection in Relation to Sex* (1871), noted that "Darwin's theory was received in Russia with great sympathy." "While in Western Europe," he said, "it encountered many established traditions and strong opposition, in our country, awakening after the Crimean War, it lost no time in acquiring the rights of citizenship, in the scientific as well as in the broader social world. The admiration for Darwin has continued to this day."

After 1883 the attitude toward Darwin underwent

perceptible change. Although admiration continued to be strong, criticism was on the rise. The Malthusian bias of the theory of evolution attracted the most poignant criticism, whose resonance reached the wide circles of the reading public. Critics of the "struggle for existence," the Darwinian metaphor for the Malthusian law, attracted the attention of all groups of intellectuals, ranging from the scientific community to the leaders of various movements led by the restive intelligentsia.

Daniel P. Todes's book has three impressive strengths. First, it presents a minute and discerning analysis of a wide variety of strategies in the interpretation of Darwin's "struggle for existence." These strategies range from extreme criticism aimed at a total demolition of Darwinism to random suggestions for minor changes intended to give Darwin's theory added strength and broader influence. Second, it offers much more than a discussion of the struggle for existence in Russian evolutionary thought. The reader is treated to a bonanza of information on the expanding compass of the scientific vision in Russia, on the more subtle aspects of the dynamics of clashing ideologies, and on the humanistic dimension of the Russian scientific community. Third, it is unusually rich in significant details on the changes and vibrations of personal views on Darwin's legacy. The author was particularly successful in portraying the salient steps in the complex history of I. I. Mechnikov's evolutionary views with specific reference to the struggle for existence.

The chapter on Mechnikov is the best and most impressive essay in the book. It shows the inordinate complexity of Mechnikov's mental make-up, temperament, philosophical alertness, and intellectual courage. A Nobel laureate, Mechnikov fought compounded difficulties not only in solving the intricacies of his experimental research designs but also in fighting vested academic interests. In his first paper, written when he was still a university student, he subjected Darwin's Malthusian premise to severe criticism. After exploring other explanations, he gradually but irrevocably sided with Darwin, becoming one of his most ardent admirers.

The chapter on Peter Kropotkin, enriched with archival material from the Hoover Library, relates the Darwinian metaphor to a strong branch of anarchist fermentation. In opposition to Darwin's struggle for existence, Kropotkin formulated a theory of "mutual aid" that, Todes tells us, had deep roots in the "Russian intellectual tradition" (p. 123). Although Kropotkin rejected the struggle for existence as a motive force of evolution, he had the greatest respect for Darwin as a man who dedicated his life to placing biology on the firm footing of modern science and who helped science establish an upper hand in areas previously controlled by metaphysics and theological thought.

From 1900 to 1915 Kropotkin published a series of articles in *The Nineteenth Century and After* aimed at

preserving and consolidating Darwin's reigning position in contemporary biology. In this grueling, ambitious, and misguided undertaking he presented Darwin's legacy as a pure and direct echo of Lamarck's theory, which regarded the direct influence of the natural environment and the inheritance of acquired characteristics as the prime movers of organic evolution, and staged a fierce attack on modern genetics and its immediate historical tributaries. A closer and critical look into this effort would have given added strength and finer balance to Todes's original and fascinating scrutiny of the broader setting of Kropotkin's preoccupation with the idea of mutual aid.

Todes's book is an outstanding contribution to the challenging and exhilarating world of Darwin studies. It is a lucidly written, precisely structured, and richly documented addition to the historical study of the social dynamics of Russian science and rationalist tradition.

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ANTTI KUJALA. *Vallankumous ja kansallinen itsemääräämisoikeus: Venäjän sosialistiset puolueet ja suomalainen radikalismi vuosisadan alussa* [Revolution and the Right to National Self-Determination: Russian Socialist Parties and Finnish Radicalism at the Beginning of the Twentieth Century]. Summary in English. (Historiallisia Tutkimuksia, number 152.) Helsinki: Suomen Historiallinen Seura. 1989. Pp. 348.

Antti Kujala labors in a much-plowed field, complementing prior work by William R. Copeland, Michael Futrell, Osmo Jussila, Tuomo Polvinen, and Robert Schweitzer. He has previously published several articles pertaining to this topic and displays a good grasp of relevant literature and sources. Kujala's broad concern is the Finnish-Russian relationship around the turn of the century, when the autonomous Finnish state resisted the threat of integration into the Russian empire. Within this larger issue he is interested in radicalism, revolutionary sentiments and activities, and motivation and aims. He deals with political radicals who were willing to resort to illegal methods. Thus, his concept of radicalism is not limited to social radicalism as such but also includes bourgeois radicals.

More specifically, Kujala explores the relationship between the most radical Finnish Constitutionalists, Activists, and Socialists, and Russian Socialists and Socialist Revolutionaries. What distinguished these radical Finns politically from their compatriots was a willingness to engage in armed struggle and rebellion to achieve their political aims if all else failed. This willingness opened the path for their cooperation with the Russian revolutionary Left.

The radical Finns and the Russian revolutionaries shared a common enemy in the tsarist regime and

from this sprung the ties between them. Yet they had different priorities and aims, and these differences put definite limits to the extent and degree of their cooperation. With the exception of the most radical socialists, the Finns' priority was to advance their national cause with the aim of achieving maximum separation from Russia. The Russian revolutionaries in contrast, of course, aimed at a revolutionary transformation of Russia. The radical Finns supported this insofar as they saw it as a means of furthering their own interests.

But nationalism does not alone explain the Finns' reservations and caution in cooperating with the Russian revolutionaries. The Finns also felt that they had more to lose than the Russian underground. Legal political activity could be carried out on an altogether different level and scale in Finland than in Russia because Finland had a legislature and a functioning legal party system, including a massive legal Social Democratic party. A wish to protect these institutions and organizational assets also added to the Finns' caution and reserve.

For the most part, the Finns shied away from being drawn directly into the Russian revolutionary struggle all the way. As an organization, the Finnish Social Democratic party stayed apart, in contrast to its engagement in 1917–18. But radical individuals in the party supported and participated in Russian revolutionary activities. They found an organizational base for such cooperation in the Red Guard, a short-lived workers militia.

Kujala tries not to repeat much earlier work that pertains to his study. This is a laudable ambition for a dissertation, but it also leaves his study a bit disjointed. Like most Scandinavian dissertations in recent years, its form and style bear a closer resemblance to a research report than to a book. There is too much explaining of the author's prior work and self-evident talk about the sources. Professional editing could have eliminated some redundancy.

Yet basically this is a sound and for the most part convincing analysis of complex matters and relationships. Kujala also makes some interesting comparative observations on Poland and commendably integrates his study into a broader perspective. He has achieved a worthwhile and welcome contribution to the field.

PEKKA KALEVI HAMALAINEN
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RICHARD PIPES. *The Russian Revolution*. New York: Alfred A. Knopf. 1990. Pp. xxiv, 944. \$40.00.

From roughly the fiftieth anniversary of the Russian revolution to the present, the study of tsarism's fall and the rise of the Bolsheviks has been enriched by a steady series of monographs, most of them based on archival sources and informed by at least a nodding

appreciation of social-historical approaches. Older paradigms informed by the memoirs of participants, a visceral anti-Leninism, and a focus on political maneuvering and personalities had dealt with the Bolsheviks as rootless conspirators representing no authentic interests of those who foolishly followed them. The new historiography argued that a deepening social polarization between the top and bottom of Russian society radicalized the lower classes, prevented the consolidation of a political consensus so desired by moderate socialists and liberals, and thus undermined the Provisional Government. Rather than being dupes of radical intellectuals, workers articulated their own concept of autonomy and lawfulness at the factory level, and peasant soldiers developed a keen sense of what kind of war (and for what regime) they were willing to fight. More convincingly than any of their political opponents, the Bolsheviks pushed for a government of the lower classes institutionalized in the soviets and advocated workers' control over industry and an end to the war. By the fall of 1917, a coincidence of lower-class aspirations and the Bolshevik program resulted in elected Leninist majorities in the soviets of both Petrograd and Moscow and the strategic support of soldiers on the northern and western fronts. After a relatively easy accession to power, the Bolsheviks, never a majority movement in peasant Russia, were faced by dissolution of political authority, complete collapse of the economy, and disintegration of the country along ethnic lines. As Russia slid into civil war, the Bolsheviks embarked on a program of regenerating state power that involved economic centralization and the use of violence and terror against its opponents.

In this mammoth history of the revolution, Richard Pipes attempts to dismantle much of this "revisionist" paradigm and breathe new life into older conceptions. Once again the major players are politicians and intellectuals, the former mostly weak-willed, the latter obsessed with power and reshaping human beings. The *ancien régime*, Pipes argues, was a patriarchal or patrimonial despotism, the appropriate government for Russia's peasants who did not crave civil or political rights. This autocratic state, dependent on its landed nobility and bureaucracy, both initiated and restrained capitalist development, which proved subversive to it. The revolution might have been avoided, however, if the state had opened up to society, but the monarchy failed to reform because of the social threats to unity and stability. Disloyal intellectuals were matched by unpatriotic peasants who tended toward primitive anarchism until the tsar had lost all support from society by the second decade of the century.

Yet, for all of its serious flaws, tsarism was a complex set of institutions built up by trial and error over many centuries, and Pipes follows Edmund Burke in holding that such structures ought not be destroyed in the futile hope that an ideal system

might be constructed. The particular delusion of intellectuals that they could bring a rational order out of the chaos of human experience eventually brought disaster on Russia. For Pipes, only the February overthrow of the tsar was a genuine revolution; October was a classic coup d'état engineered cynically by power-hungry conspirators, led by the cowardly Vladimir Lenin. Once in control of the state, the Bolsheviks ruthlessly used all of their instruments to realize the imperatives of Marxist ideology, which included the destruction of multiparty democracy and the market system, the introduction of compulsory labor, and a war against the peasantry.

From the grandiose claim in the first sentence—"this book is the first attempt in any language to present a comprehensive view of the Russian Revolution" (one wonders what Leon Trotsky, William Chamberlin, Marc Ferro, John Keep, and numerous others had in mind)—the book promises more than it delivers. With cavalier disregard of the major arguments of those who have written on the revolution for the last twenty-five years, Pipes places himself outside (or above) the professional discourse and contemptuously rejects what he refers to as "the mental straight-jacket that seventy years of politically directed historiography have managed to impose on the profession" (p. xxiii). Despite the book's bulk, there is little space for analysis, for the text is largely a detailed narrative of selective episodes. An entire chapter, fascinating in its macabre detail, is devoted to the execution of the imperial family, followed by a lengthy treatment of the "Red Terror," which centers on the attempted assassination of Lenin. But one looks in vain for equivalent treatment of the millions of workers and soldiers (the peasants fare better) who made the Bolshevik victories possible both in 1917 and in the Civil War. This book is also not the place to seek any understanding of the White forces that faced the Reds or any mention of the White Terror that provided for many the justification for the harsh policies of the new Soviet state. Entire social groups, classes and nationalities, and women and workers are crowded off the historical stage and replaced by an eclectic cast of characters that includes a few heroes such as conservative politicians Petr Stolypin and Aleksandr Guchkov, a large number of inept actors such as Aleksandr Kerensky and the Mensheviks, and the pathetic protagonist of the tragedy that he did so little to prevent, Nicholas II.

Rather than providing a synthesis of what we know about the revolutionary processes of 1917–18 or a reinterpretation that contends with the major contributions of recent historiography (almost none of which is even referred to in notes or bibliography), Pipes has offered a personal political vision, an indictment that is highly selective, uneven in its treatment, and eccentric in its emphases and omissions. Although there is little new to offer to scholars working in the field, Pipes's argument that the Bolshevik regime was unpopular and illegitimate from its incep-

tion will find a ready audience, both among those in the West celebrating the demise of what they understand to have been socialism and among those in the Soviet Union who believe that their recent past can be completely expunged.

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LARS T. LIH. *Bread and Authority in Russia, 1914–1921*. (Studies in the History of Society and Culture.) Berkeley and Los Angeles: University of California Press. 1990. Pp. xvii, 303. \$38.00.

The jacket of this thoughtful study by Lars T. Lih reproduces a Russian poster showing the “bony finger of Hunger” pointing to starving masses. Food supply was a critical challenge facing all Russian governments throughout the strife-torn period from 1914 to 1921, and the bony finger threatened regimes as well as the populace they ruled. Lih’s objective is to demonstrate how the food problem was handled and to what political effect. Drawing on archival sources, monographs, memoirs, and novels, he offers an analysis of successive supply policies and an interpretation of their political significance. The book is aimed at specialists and includes a useful bibliography.

At the core of Lih’s concerns is Thomas Hobbes’s dilemma: the choices faced by individuals confronting a shaky sovereign authority. According to Hobbes, individuals are ordinarily obliged to submit to state authority, and this helps to secure social order. Yet the particular interests of individuals may not be served by the policies of the state, which is pursuing its own interests and those of society as a whole. Only in periods of transition from one political system to another, when the old is discredited and disarmed and the new not yet firmly in control, can individuals choose whether to support the system or pursue their private interests. The choices they make, says Lih, affect the balance of disintegrative and centralizing forces in play during “times of trouble” and determine the outcome.

As Lih sees it, Russian political culture offered two possible solutions to the supply problem: a “gubernatorial” approach, based on firm central control and taxation, or an “enlistment” strategy, drawing broad public sectors into the policy process but in vertical hierarchies supporting a state monopoly in food. Each strategy had its advocates, but the distribution of political forces prevented the consistent implementation of either.

It is no simple matter here to follow lines of policy or their partisans. Part of the difficulty lies in the nature of the period and part in its shifting rhetoric, about which Lih offers some perceptive observations. But part may be because the alternatives counterposed here are theoretic constructs that found no clear expression in the complex realities of these

years. It could be argued that the “two solutions” were neither mutually exclusive nor the only ones possible, even within the constraints of Russian political culture.

Interestingly, Lih suggests that the complexities of the period, the tangled web of interlinked and often simultaneous crises, might best be presented in “some equivalent of a split-screen effect” (p. 57) and with animation. This conceptualization, a product of the video generation, is an advance over traditional sequential imaging, and it raises intriguing—if peripheral—questions about the role of the “new looking” in stimulating new thinking.

The notion of history as camera returns to the reader when the text zooms in for conclusions. The final and sharply focused chapter of this tale of trials and errors, crisis and confusion, unexpectedly presents the reader with a shower of charts and statistical tables (seventeen of the eighteen in the book) and a suddenly crystallized conclusion about the logic of the Bolshevik policy of *razverstka* (requisition of grain from the peasants). Although many have viewed that policy as the cause of a disastrous drop in agricultural output, Lih argues, with supportive evidence, that other factors (lack of exchange commodities, shortages of agricultural equipment, and so forth) were responsible for the decline. In his view, grain requisitioning helped to make possible the reconstitution of a single political authority, of the state, and of society. That achievement, he concludes, should command our respect.

The *razverstka* policy was “gubernatorial”; it meant centralized control. According to Lih, the Bolsheviks moved from a policy of “enlisting the population in a revolutionary crusade to building up an apparatus that would earn respect and compliance by its competence and staying power . . . [They] realized that the confident use of force attracted support” (p. 197).

Doubtless, the prospect of stability and order—even imposed order—was attractive under the circumstances. And the observation that the Bolsheviks were obliged to abandon their confiscatory policy in 1921 does not refute Lih’s argument. Nonetheless, the implications of the analysis, like Hobbes’s view of human nature, are far from cheering.

There are disquieting similarities between the period discussed in this work and the present time of troubles in the Soviet Union: intense political strife, economic dislocation, and widespread food supply problems. Now as then a search for security, stability, and the satisfaction of basic human needs agitates Russian society and impels it toward hard choices, Hobbes’s choices. Lih’s discerning and sympathetic analysis enlarges our view of both past and present.

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CHRISTOPHER READ. *Culture and Power in Revolutionary Russia: The Intelligentsia and the Transition from Tsarism to Communism*. New York: St. Martin's. 1990. Pp. xii, 266. \$55.00.

In this well-researched and well-written book, Christopher Read convincingly challenges the traditional interpretation of the New Economic Policy (NEP) in culture as a Bolshevik "soft line" on culture and education. He argues that by 1922, not 1932, the cultural policies of the Soviet government had already emerged. In his view, NEP was not a period of cultural relaxation and toleration but instead a step toward Stalinism in culture. Like many Western scholars who have recently written about the Russian revolutions and the Soviet 1920s, Read, too, focuses on the "formative experience" of the Civil War to discern basic Bolshevik principles on politics and culture. He holds that the Civil War witnessed arbitrariness and capriciousness in culture, which gave way to increasing central control in the early 1920s. He rejects the centralization-liberalization-centralization model frequently associated with the Civil War-NEP-Stalinism. In contrast, Read proposes arbitrariness-limited control-rigid control with the cultural revolution of 1928-32 as the watershed between the latter two terms.

Read correctly indicates that the Bolsheviks ultimately wanted to transform human nature. Bolshevik leaders such as V. I. Lenin, L. D. Trotsky, and N. I. Bukharin were initially preoccupied with pressing political, military, and economic problems. Other figures, party and nonparty as well, such as A. A. Bogdanov, A. V. Lunacharsky, M. Gorky, and N. K. Krupskaya, were deeply interested in culture and enlightenment from the beginning of Soviet rule. According to Read, the foundations of Soviet cultural policy were established between 1920 and 1922 as the party at the highest level intervened in culture. Much of the debate on the methods of reshaping human nature centered on the intelligentsia, whom he describes as the culture bearers possessing a critical spirit. He thinks that the Civil War shattered the fragile unity of the intelligentsia and resulted in the attainment of their specific class consciousness.

Read presents an especially good analysis of Narkompros and Proletkul't. The two organizations represented an interesting contrast: whereas Narkompros had organizational plans but no lucid vision of the role of culture in the transition to socialism, Proletkul't lacked a coherent organizational structure but relied on Bogdanov's vision of proletarian culture. In the end, the party was the winner, although it failed to develop both organization and vision during the Civil War. Read believes that by 1922 the Bolsheviks had already completed a revolution in culture through institutions of control concentrated on the party Central Committee.

Even though Read has extensively used Soviet archives, he has not brought any new documentation

to bear on the question of culture and politics in the 1920s. But he has gathered an impressive body of Soviet primary sources and Western and Soviet secondary works to create a fresh, stimulating approach to the Bolshevik revolution in culture. He is correct to insist that NEP in culture must be understood not only in comparison with what followed it, the Stalinist revolution, but particularly in comparison with what preceded it, the Civil War as the period in which Soviet cultural and political institutions and principles were formed. This highly analytical monograph is indispensable reading for specialists on the Soviet 1920s.

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GRAEME GILL. *The Origins of the Stalinist Political System*. (Soviet and East European Studies, number 74.) New York: Cambridge University Press. 1990. Pp. xv, 454. \$64.50.

The study of Stalinism is not enviable work. First of all, the sources accessible to Western historians have been severely restricted. Second, the field is politically and emotionally charged, so much so that whatever one says, one is destined to be damned by someone. The recent changes in the Soviet political climate have not discharged the tension but they have allowed far greater access to the archives than before and have therefore benefited the community of historians.

This book by Graeme Gill was written with only minimum benefit from glasnost, drawing largely on a basic range of published documents and the vast secondary literature on the formation of the Stalinist political system. Parts of the book are tedious, but Gill has managed to write a judicious and balanced account of how Joseph Stalin's personal dictatorship was established in the period from 1917 to 1941. Gill addresses one of the most controversial issues of Stalinism: whether or not Stalinism was an inevitable outcome of Leninism (or the October revolution of 1917). He supports the discontinuity position. Although "the roots of the Stalinist system are to be found in the Leninist, the roots of other paths of development are also present in this early stage." Therefore, the task is to explain "why, at crucial stages, these other potential paths were closed off" (p. 307).

According to Gill, there were several such stages: the succession struggle of 1922-24, Stalin's "revolution from above" in 1928-29, the famine and post-famine crisis in 1933-34, and the Great Terror in 1937. Gill carefully analyzes the four periods in detail. At each stage, Gill maintains, two factors worked against the integrity and coherence of political institutions and for the personalized, patrimonial power structure. The first factor, which is contextual, was the fear of a hostile environment, both domestic

and external. What was important was not so much the environment itself as the leadership's perception of the weakness of the Communist party's political base. At each stage, this fear allowed a concrete opposition to give substance to a metaphysical one. The political base of opposition was analyzed in class terms according to the ideology of the party, and the apprehension of nonproletarian influences prompted the party to adopt not an inclusionary but an exclusionary policy. This process culminated in 1937 with Stalin's all-out attack on the party.

The contextual factor combined with a second factor to lead the party in the direction of centralization: the interests of the party rank and file, the subnational party bosses, and the party elite converged to favor the personalized structure of power. The poorly educated rank and file, according to Gill, supported monolithism and discipline. The subnational party leaders, jealous of their power and anxious to protect themselves against attack, rejected forsaking the personalized basis of power in favor of formal institutions and rules. They also supported the intrinsic interests of the party elite in Moscow in the centralized power structure because centralization freed the subnational leaders from rank-and-file control. An obvious tension existed here between the center and the periphery, which Stalin sought to eliminate by the Great Terror in 1937–38. The result of these processes was the establishment of Stalin's personal dictatorship.

Despite focus on the political environment and the party, Gill contends that Stalin's personal role was crucial at each stage. Unlike those who regard Stalin as the determinant of Stalinism, Gill views Stalin's actions not in a political vacuum but places it within the political context.

All in all, Gill presents a sophisticated analysis of the formation of the Stalinist political system. From a historian's point of view, Gill's focus is too narrowly political. As Gill emphasizes, the phenomenon of Stalinism is "totalist." Its social, economic, and cultural aspects are not treated here, but perhaps that is the task of historians.

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DMITRII SHELESTOV. *Vremia Alekseia Rykova* [The Time of Aleksei Rykov]. Moscow: Progress. 1990. Pp. 349. 1 r. 90 k.

Dmitrii Shelestov has four goals, two stated and two unstated: to shed light on important Old Bolsheviks through biography, in this case Aleksei Rykov (pp. 20–21); to help restore all of Joseph Stalin's victims, in common with other biographies of repressed leaders (p. 12); to show that Rykov, who "demonstrated sharp reality" in regard to building socialism (p. 17), was a forerunner of Mikhail Gorbachev; and to

debunk Stalin. After forty-five pages of preliminaries, the book is a straightforward chronological study of Rykov's life and times.

Shelestov's main thesis in this first Soviet study of Rykov is that Rykov was "one of the most important expressers of that tendency in the Party" that sought "a bloodless, peaceful, economic, pedagogical, and administrative [in the positive sense] approach toward the dictatorship of the proletariat" (p. 240); this was possible because Rykov was an outstanding representative of the Russian intelligentsia (p. 221). Shelestov restores Rykov to his rightful place in Soviet history, arguing that Rykov was left out of V. I. Lenin's Testament only because Lenin had discussed Rykov's abilities in letters written somewhat earlier (p. 196). The period after 1925 is rightfully the Stalin-Rykov period, and Rykov, "Lenin's successor" (p. 307), exceeded Nikolai Bukharin in importance (p. 238). This is not old-style Soviet hagiography. Shelestov discusses some of Rykov's differences with Lenin but correctly points out that these occurred within the framework of Rykov's positive Bolshevik activities. The author criticizes Rykov for his errors, as in 1917, and of naïveté in the struggle for power (pp. 213–14, 311–12). Rykov emerges in this study as a pre-Gorbachevite. Thus, in discussing the Eleventh Party Congress, Shelestov emphasizes separating the party and government (p. 191). We see this also in emphasis on Rykov as a supporter of friendly U.S.–Soviet relations, equality for all Soviet nationalities, and support for the intelligentsia and *spetsi* (pp. 251–55, 269, 137). Rykov, Bukharin, and Mikhail Tomskii were "right" in the 1920s, but not as Stalin understood that term (p. 269). Finally, the real villain was Stalin, "a murderer" (p. 260) who distorted the concept of the leading role of the party into the dictatorship of the party dominated by himself (p. 249). Stalin was the factionalist, not Rykov and his allies (p. 268), and Stalin was responsible for the disasters of the 1930s (pp. 291–92).

Shelestov adds a personal dimension and certain specifics of political events to Rykov's life heretofore unavailable, the result of access to archival materials and contact with Rykov's daughter Natalia. Unfortunately, quotations are seldom footnoted. Shelestov adds little of political substance and tends to gloss over Rykov's differences with Lenin (as in 1917), underplay them (Rykov's reconciliationism before 1912), or ignore them (the trade union issue after 1917). Regarding sources, he incorrectly states (p. 148) that there is little extant work on Rykov and none on his role as *Chusosnabarm* (Extraordinary Plenipotentiary of the Council of Defense for Supplying the Red Army and Navy). Relying exclusively on Soviet sources, Shelestov ignores my own work since 1973: a full-length biography (*The Practical Bolshevik: A. I. Rykov and Russian Communism, 1881–1938* [1979]) and articles in *Soviet Studies* ("The Supreme Economic Council, 1917–21," 25 [1973]), *Slavic Review* ("The Making of a Right Communist—A. I. Rykov to 1917,"

36 [1977], and *The Modern Encyclopedia of Russian and Soviet History* ("Chusosnabarm," vol. 7 [1978], "Reconciliationism," vol. 30 [1982], "Right Communism," vol. 31 [1983], "Rykov, Aleksei," vol. 32 [1983], and "Supreme Economic Council," vol. 38 [1984]).

Shelestov's history is both current and outdated. Thus, he questions whether General Denikin or the Chekists did more damage and harm (p. 159). Yet Lenin remains above reproach (pp. 105–12), and the correctness of Marxism-Leninism is not questioned (pp. 56, 61).

The book is successful for the Soviet reader interested in a more objective view of Soviet history. To the Western reader the work offers little that is new other than in terms of Rykov's personal life. I recommend it to those interested in the newer approach in Soviet historiography and a Soviet interpretation of Rykov's life.

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MARC RAEFF. *Russia Abroad: A Cultural History of the Russian Emigration, 1919–1939*. New York: Oxford University Press. 1990. Pp. viii, 239. \$29.95.

Immigration-emigration-assimilation studies have developed their own scholarly dances that are not always satisfactory to specialists whose interests cross just one corner of the stage. The variables involved in economic, cultural, and political motives for emigration, complicated by corresponding problems of assimilation and ravaged by considerations of mortality and generational change, have produced an enormous variety of approaches and theories. Specialists in a given national group frequently prefer to emphasize just the peculiarities of their own subject.

Marc Raeff deals with "Russia Abroad" more or less as a cultural unit, a diaspora extending across complex international boundaries. He refuses to consider the political history of the emigration—"it is not very profitable to try to disentangle the complicated and sterile political factionalism of Russia Abroad" (p. 9)—and concentrates on the institutions, ideas, and cultural values that united Russians from Harbin to New York in the 1920s and 1930s. He means specifically the Great Russians, with their language and Orthodox religion, and he devotes considerable space to their values, ranging from their views of Fyodor Dostoyevsky to positivist historiography and Eurasianism. The text moves easily from Nikolay Berdyayev to Pavel Miliukov to Igor Stravinsky. Raeff's purpose is "to account for the makeup and creative life of a society in exile" (p. 187). The result is a virtuoso performance.

At the same time, one might cavil about what is not in the book. Raeff's dismissal of political strife produces an unsatisfactory image, for example, of what effect the "Change of Signposts" (*smena vekh*) move-

ment might have had or of how émigrés might have chosen to live in Berlin, Sofia, Belgrade, or Paris depending on their own political views. On another level, the study's use of the terms "exile" and "émigré" seems too casual. "Exile," all romantic images aside, might best be left to characterize those who, like the group of intellectuals in 1922, were specifically forced to leave Russia, whereas "émigrés" perhaps experienced a more orderly departure than "refugees," who fled danger.

Raeff argues that Russia Abroad was a "unique phenomenon" (p. 193) and that it no longer exists. The advent of Stalinism in the Soviet Union gave Russia Abroad a vital mission to preserve Russian culture at a time of severe repression at home. It remains to be seen how its accomplishments will be integrated into the mainstream within Russia. Recalling myself how my mother took me as a child to hear Serge Jaroff's Don Cossack chorus and reared me with Aleksandr Kipnis's folk songs, I must confess to having deeply enjoyed the trip through these pages.

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NEAR EAST

BERNARD LEWIS. *The Political Language of Islam*. Chicago: University of Chicago Press. 1988. Pp. vii, 168. \$14.95.

About forty years ago, Bernard Lewis began to write about the intermingling of "religion" and "politics" by Muslims. A few years ago, he debated with Edward Said, who has criticized Orientalists for constructing "Islam" as impersonal actor and timeless abstraction. So it is natural to ask what Lewis means by "political" language and how he has constructed "Islam" as its *location*.

That this book explicitly addresses neither question partly reflects its origins in a series of lectures and its brevity. Presentation of individual points is often fresh, lively, and insightful, especially in matters of etymology and semantics. Yet, because Lewis is not explicit about his approach and methodology, the validity of his interpretations and generalizations is clear only to the specialist.

The book's geographical limits are specified—the Arabic, Persian, and Turkish-speaking Muslims of the Middle East. Much less explicit is its concentration on Sunni points of view rather than Shi'i or Khariji, on theory rather than practice, on texts more than on institutions or actors, and on written rather than spoken language. Lewis acknowledges that the majority of Muslims live "outside the Middle Eastern Islamic heartlands. . . , [b]ut they have developed their own political and other cultures, much influenced by those of the regions in which they live. Their relevance to the Middle Eastern culture is minimal, and they are therefore not considered here" (p. 6). In this

rationale, and in his stated and unstated limits, Lewis reflects Said's characterization of Orientalism as well as the views of many living Muslims. "Islam" is an actor, a place, a location, an abstraction. Paradigmatic, or normative, "Islam," and its "classical" foundation, is Middle Eastern, Sunni, and textual, and the "Islam" of that region, unlike others, can be disentangled from its local cultures.

If, however, the political language of Islam is not necessarily the political language of all Muslims, then is there "non-Islamic" political language "among" Muslims, or "in" Islam, and is a Muslim view the same thing as an Islamic view? If, as Lewis demonstrates, "Islamic" language can be found in nonreligious sources, can non-Islamic language be found in texts written by Muslims? If religion and politics are mixed, is there any "religious" language that is not "political"? Lewis's answers are unclear. For example, he does not elaborate significant religio-political terms and institutions such as *zandaqa* (heresy) and *da'wa* (revolutionary mission). Neither does he dwell on the "circle of justice," a "political" image of absolutism favored by Muslim rulers and writers but not necessarily by religious thinkers. Without a sense of the absolutist tradition, one might easily be misled by Lewis's claim that neither in theory nor in practice is "Islamic government" the rule of an all-powerful despot over a slave subject (p. 31).

Lewis's self-imposed limits and his usages of "Islam" and "Islamic" render many generalizations problematic. For instance, although there are certainly strong iconoclastic traditions, especially among Sunni Muslims (p. 10), they are not always operative, and there is religious art and music outside the mosque context for Sunnis as well as Shi'is. Moreover, Lewis asserts that Khomeini's "authority of the faqih" has no precedent in "past Islamic doctrine and practice." In making his assertion, Lewis uses a Sunni definition of *faqih* (legal scholar), and characterizes Khomeini's concept as providing "for the establishment of a single *faqih* as the supreme legal authority in the state" (p. 28), who can invalidate non-Islamic government actions. In fact, Khomeini's *faqih* is more than that, partly because the Shi'i definition includes powers of charismatic exegesis absent in the Sunni *faqih*. Furthermore, even if Sunni doctrine contains no precedent, Shi'i doctrine does, because the concept of *faqih*-ruler was being debated by Iranian Shi'is by the early nineteenth century.

Lewis's reliance on textualized language raises a problem not limited to the Orientalist but in fact common to many historians of ideas. Accounts of a particular "position" or "tradition" often rest on a treatment of texts from different genres and provenances as if they are interconnected and relatively context-free. In chapter 5 Lewis analyzes as a "succession of jurists and theologians" (pp. 101-02) three figures (ibn Batta, al-Ghazali, ibn Jama'a) whose historical interconnections are unclear. They may not even have read each other or had the same readers,

and their opposition to rebellion may not represent varying positions on the same issue or "Islamic principle" as much as practical politics and engaged responses to individual circumstances. And, if the norms of Islam encouraged obedience, why did so many men of religious learning resist state employment so fiercely, and why were there so many rebellions, especially ones with religious leaders? And although Lewis's comparisons of Islamdom and Christendom are welcome, can one draw a sharp contrast between the Islamic view of political authority as a divine good and the Christian suspicion of the state as a human evil (p. 25)? That is to say, can we really talk about the "political language of Islam" without designing a new language for talking about it?

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JEREMY WILSON. *Lawrence of Arabia: The Authorized Biography of T. E. Lawrence*. New York: Atheneum. 1990. Pp. xi, 1188. \$35.00.

This work by Jeremy Wilson is a monumental piece of historical research. Running to almost one thousand pages of text, with more than one hundred pages of footnotes, the opus is a complete and closely argued biographic account of perhaps the most publicized and controversial British official involved in the Arab revolt against the Ottomans during World War I and the struggle for Arab national independence. The book also includes an extensive note on Lawrence's ancestry, several complete reports on the Arab world by Lawrence, and a complete list of Lawrence's published writings. For such a thorough study, it is unfortunate that Wilson has included the secondary works within the long footnotes rather than in a formal bibliography. A bibliography could have provided a definitive and presumably lengthy list of the available published material dealing with Lawrence.

Importantly, Wilson has not written just a narrow biographic account but has placed Lawrence's activities and contributions within the larger historical context of the war and the rivalry among the great powers. By including long excerpts from Lawrence's published works and voluminous correspondence, Wilson has wisely chosen to let Lawrence speak for himself whenever possible. Wilson has cast his net so far and so broadly, however, that in the welter of detail the reader may well lose sight of Lawrence as an individual. This is particularly true in the key chapters involving Lawrence's relationships with Arab leaders and his role regarding both the political and the military aspects of the Arab revolt.

Wilson makes excellent use of a daunting volume and array of documents, letters, and memoirs to detail Lawrence's early life, intellectual development, and work in the Arab world. In the process, Wilson

not only plows new ground regarding Lawrence's rather enigmatic life but also refutes the many errors of fact and analysis in other biographies of Lawrence and historical accounts of the Arab revolt. For example, Wilson points out that Elie Kedourie's studies on Arab nationalism, the conquest of Damascus, and British policies are weakened, if not considerably undermined, by Kedourie's emphasis on reports from officials in India over those from Cairo and by the fact that the essay on Damascus was written before all of the documents were made available. Similarly, Wilson uses his thorough scholarly research to decimate the biographies of Lawrence by Robert Graves, Lowell Thomas, and others.

Wilson purposely avoids sensationalizing any aspect of Lawrence's life but does not flinch from the questions regarding Lawrence's psychological motivations and sexuality. Wilson emphasizes that the evidence does not indicate that these issues were central to Lawrence's support for the Arab revolt and his contributions to it. Thus, although Wilson frankly describes the incident of homosexual rape at Deraa and its subsequent long-term effects on Lawrence, he does not make it the centerpiece of the long narrative on the extremely convoluted and contradictory policies adopted by the British government toward the Arab world during and after the war.

Unfortunately, the study is not so incisive in addressing what can only be described as Lawrence's "Orientalism," that peculiar blend of romanticism, exoticism, and love and hate toward the Arabs and other non-Western, and in this case specifically non-British, peoples with which so many were imbued. Wilson himself notes that Lawrence's aim was "noble and romantic" (p. 543) but then fails to emphasize that his simultaneous encouragement of Arab national independence and participation in forwarding the conflicting imperial aims of the British government in the region can be understood only within this context. In short, Lawrence's vision of the Arab world was of his own making. He sought his own particular formulation of Arab independence, not necessarily what the Arabs wanted for themselves. In addition, Lawrence was first and foremost a British patriot; in the final analysis he rationalized what he recognized as his own duplicity as necessary for the greater good of British victory. Thus, although the book is undoubtedly a tour de force in terms of historical research and synthesis of biographic material, it is not, as some reviewers have seemed to suggest, the definitive account of Lawrence as the molder of an effective Arab national movement. Only a study focusing on Arab views regarding not only Lawrence but also Zionism, the decline of the Ottoman empire, and the British and French as imperial powers can place Lawrence's true role regarding Arab nationalism and his contributions to the Arab revolt in the proper perspective.

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DAVID DEAN COMMINS, *Islamic Reform: Politics and Social Change in Late Ottoman Syria*. (Studies in Middle Eastern History.) New York: Oxford University Press. 1990. Pp. viii, 199. \$32.50.

While the study of Arab social and economic history has flourished in recent years, intellectual history has been rather neglected. David Dean Commins performs a valuable task in helping to bring the two fields together by analyzing the social and intellectual history of Damascus religious reformers during the period 1885 to 1914.

Commins carefully traces the reactions of the professional men of religion to three trends that threatened their status: the secularization fostered by the Ottoman central government, the emergence of Arab secular nationalism, and the spreading influence of Europe. Religious reformers seeking to overcome these threats wished to return to original scriptural sources. The reformers rejected imitation of the medieval commentators, hoping thereby to regain the strength and unity of early Islam. Through fresh interpretation of basic theological sources, the reformers believed they would become an elite qualified to render judgment on new religious, social, moral, and political issues; other scholars who continued to follow earlier interpretations should lose their right to hold religious-legal offices.

This elitism was one of the reasons for the weakness of the reformers, who numbered only a handful. Other causes for their relative lack of success included their marginal position in the modernizing society of late Ottoman Syria, opposition from conservative men of religion, and occasional intimidation by government officials. Despite these problems, the reformers influenced Arab nationalists and even some religious conservatives by showing how European modernity could be compatible with Islam.

The religious reformers of Damascus have been set in their historical framework in a splendid fashion by Commins, who tells how they were linked with a large variety of other groups such as the men of religion of earlier times, the Ottoman central authorities, secular school graduates, reformers in other provinces, Shi'is, and the mystical orders. Most remarkable among these linkages is the author's systematic and convincing discussion of the conservative opposition to the reformers. Commins also painstakingly traces the tangled relationship between Islamic reformers and nationalists, in the process casting much light on the intellectual origins of the nationalists. Despite the book's brevity, there is also information on many other matters, including the structure of elite families, law, the status of women as viewed by men, censorship, and government control of intellectuals.

This book is based on an impressively thorough examination of private family papers, Ottoman inheritance court registers, a vast array of manuscripts and published Arabic-language materials, appropriate Western works, and the British archives. Unfor-

tunately, the author seemingly did not consult the Ottoman central archives in Istanbul.

In demonstrating the causal linkages between social history and intellectual history in nineteenth-century Damascus, Commins has built on earlier work by John O. Voll, Albert Hourani, Linda Schatkowski Schilcher, and Philip S. Khoury, but he also makes an important and original contribution of his own. Commins provides a definitive picture of Islamic reformist thought and its social origins in Damascus; in doing so he whets the reader's appetite to see the same analysis applied in other places and times. If books of this quality appear for such cities as Beirut, Baghdad, Jidda, and San'a, we shall finally be able to understand the modern history of the religion of Islam in the central Arab lands of the Middle East.

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DANIEL PIPES. *Greater Syria: The History of an Ambition*. New York: Oxford University Press, 1990. Pp. viii, 240. \$29.95.

This book is reminiscent of George Kennan's famous "X" article that warned of the domestic roots of Soviet expansionism. Syria's Hafiz al-Asad regime, Daniel Pipes argues, has pursued an expansionist pan-Syrian policy abroad meant to legitimize authoritarian and minority rule at home.

Pipes first argues that the real identities of Syrians are communal or sectarian. But pan-Syrianism rather than pan-Arabism has been the dominant ideology manipulated by politicians. Although Pipes acknowledges that only the small Syrian Social Nationalist party embraced the idea of a distinct Syrian nation (what he calls "pure Pan-Syrianism"), he argues that the main practical aim of Syrian leaders has been the reconstruction of greater Syria; they are hence labeled "pragmatic Pan-Syrianists." The problem with this argument is that for most Syrians, including the political elite, the unit of national identity is Arab, not Syrian. Greater Syria is a component of a larger Arab nation, and while most Syrians no longer believe a united Arab state is practical, they do see Syria's special identity as the most Arab of the Arab states: "pragmatic Arabism" appears a more appropriate description of this identity.

In the second part of the book, Pipes argues that the current regime, being unalloyed sectarian rule, needs an irredentist ideology to divert attention from internal oppression. Because the Alawi elite, being non-Muslim, has no identification with Arabism (seen as a cover for Sunni rule), it secretly embraces a pan-Syrianism more compatible with its minority character. This argument reduces Syrian politics to a single dimension, sectarian conflict, to the neglect of equally crucial factors including class, agrarian populism, and reason of state. Pipes's own text, however, suggests that loyalties are really much more complex

and multiple than he concludes. In reducing Syrian foreign policy to mere legitimizing irredentism, he discounts the real grievances and security fears that have shaped it. As his able recounting of the origins of these grievances should have made clear, nationalist policies are more a response to popular expectations than the invention of external enemies for domestic consumption. Although Pipes reduces pan-Syrianism to a mere cloak for sectarian ambitions, he nevertheless believes it makes Syria a threat to its neighbors. Yet he admits that Asad has sought influence rather than conquest. The fact is that Syria's policy was determined less by some ideological imperative to incorporate its neighbors than by a drive to build a regional bloc for containing Israeli power. A more persuasive analysis of Asad's foreign policy is found in Patrick Seale's *Asad of Syria: The Struggle for the Middle East* (1988) which sees Asad as a nationalist defending Arab security against Israel and its Western backers.

Pipes presents his book as a novel interpretation, but it breaks no new empirical ground despite extensive citations. The book's one-sidedness may result in part from his frequently uncritical use of unreliable sources, rumor, and propaganda, and his apparent lack of first-hand experience with the country. Pipes's method is to construct a mosaic of quotations—an intellectual history of an ideology—rather than to place data in a historical context or conceptual framework that would help explain the complex permutations of identity in Syria. The book seems to me chiefly a policy paper seeking to show that Syrian designs, responding to no authentic interests or real threats, should be contained rather than accommodated in any resolution of the region's conflicts.

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EHUD R. TOLEDANO. *State and Society in Mid-Nineteenth-Century Egypt*. (Cambridge Middle East Library, number 22.) New York: Cambridge University Press, 1990. Pp. xiv, 320. \$54.50.

In writing this book, Ehud R. Toledano has two main objectives: to alter the received image of Abbas I, ruler of Egypt between 1849 and 1854, as withdrawn, cruel, and reactionary, and to demonstrate the pivotal importance of his reign for nineteenth-century Egypt. By constructing a new Ottoman-Egyptian elite and building up dynastic power, Abbas, the author contends, definitively ensured the survival of the regime established by Muhammad Ali. Toledano also argues that the years between 1841 and 1863 form a distinct historical period that was quiet and relaxed when compared with the decades preceding and subsequent to it, and he reminds us of the continuing Ottoman influence in the administration by, among other things, using Ottoman-Turkish spellings of

well-known proper nouns (thus, Said becomes Sait). This book, however, is not a study of the 1841–63 period. Toledano focuses almost entirely on Abbas's reign (scant attention is given to the reign of Said) and is concerned chiefly with the power state, that is, with the ruler and the governing elite, and the relationship between the central government and Egyptian (especially urban) society.

The originality of this study derives from the author's use of documents from the Cairo archives that produce a new picture of Abbas. Though severe, Abbas was certainly not cruel, nor was he a reactionary. His approach to reform was conservative (he wished to rely less on the European element for Egypt's development, proceed more slowly in general, and forge closer ties with the Ottomans while preserving Egypt's autonomy). Abbas tended to business and showed a concern for Egypt. He released the urban population from conscription and removed burdens on the weaker elements in society. In general, his reign was a less burdensome time for Egyptians than was Muhammad Ali's period. The "demon image" of Abbas, created by spiteful members of Muhammad Ali's family and by some Europeans, is thus put to rest.

Toledano, however, magnifies Abbas's contribution to history. Abbas did not create the new Ottoman-Egyptian elite, which fully emerged only in the mid-1860s (when the viceregal princes no longer filled key administrative posts) and which was the result of a long process to which Muhammad Ali's educational program and Said's elevation of Egyptian *umdahs* also contributed. True, Abbas brought into high office a number of Egyptian and Turkish-speaking technicians, but he also had a mamluk corps (phased out under Said) at the center of government. (Abbas was the last ruler in his line to have such a power base.) Under Said, members of Muhammad Ali's family were temporarily returned to high administrative posts. The Ottoman-Egyptian elite, then, was by no means stabilized or made permanent by Abbas.

Neither did Abbas establish the dynastic legitimacy of Muhammad Ali's family, a process to which other rulers also contributed. Oddly, his one really important act in establishing dynastic order—successful resistance to Ottoman efforts to curtail Egypt's autonomy—receives much less attention than other relatively unimportant matters (for example, palace construction, court ceremonies). As for Abbas's "alternative" approach to reform, this, too, is exaggerated. Abbas was not opposed to European-inspired reform (he granted the railway concession to Britain), as Toledano himself admits. And had Abbas lived longer, it is doubtful that he could have resisted growing European pressures to "open up" Egypt. In any case, slower-paced change was a situation he inherited rather than created as a matter of policy. Said's accession did, of course, lead to a reacceleration of the reform process (including the arrival in Egypt of large numbers of Europeans), and contemporary

observers correctly drew parallels with Muhammad Ali's time. Abbas's reign thus remains an interregnum between two more active and important periods of history.

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DONALD MALCOLM REID. *Cairo University and the Making of Modern Egypt*. (Cambridge Middle East Library, number 23.) New York: Cambridge University Press. 1990. Pp. xviii, 296. \$54.50.

A glaring omission in the abundant historiography on modern Egypt has been work on higher education. Despite its centrality to twentieth-century Egypt and, indeed, to education in the Arab world, Cairo University has lacked an authoritative history. Now Donald Malcolm Reid, drawing on his deep understanding of the intellectual development of the country, has closed this gap. Reid's book provides a definitive interpretation of the foundation and early evolution of the university. At the same time it leaves ample room for further investigations into the university's powerful social impact. Reid covers the first four decades of the university's existence (1908–52) with a deftness and clarity that enhance our knowledge of the major cultural and political developments of that period. Less effective are his chapters on the Nasser years and after, by which time the sheer size of the student population and the multiplicity of educational programs had taken the university into a formidable niche in modern Egypt.

Cairo University owed its beginnings to demands made by intellectuals and nationalists at the turn of the century. When the British proconsul, Lord Cromer, procrastinated, Egyptian leaders established a privately endowed school. In spite of the fact that Jurji Zaydan, Sa'd Zaghlul, Qasim Amin, and Mustafa Kamil had originated the vision, the university's governing council was quickly dominated by men of the palace. Using Cairo University archives and drawing on Arabic studies of the school, Reid gives a wry account of the traumas of these early years. Unable to find native-born Egyptian faculty, the university had to draw its teachers from Europe. Some of the most effective and distinguished teachers were in fact European Orientalists, pleased by the prospect of residing in Egypt and successful because of their knowledge of classical Arabic.

In 1925 the university became a public institution. The author uses his intimate knowledge of Egypt's intellectual life and his sensitive understanding of the university's leading figures, Taha Husayn and Lutfi al-Sayyid, to trace the evolution of the school from then until 1952. In these deeply researched and elegantly written chapters, Reid brings to life the whole range of Egypt's interwar cultural life.

Until the 1950s Cairo University operated on a small scale; its impact on the larger society was

limited. To be sure, struggles between secularism and Islamism occurred on the campus and nationalist protest took on extremist forms there in the 1930s and 1940s. But it was not until the Nasser government opened wider the doors of the university that higher education and its students could forge a dominant place in Egyptian society. Only fragments of this story are narrated: a nod to Islamic politics, a brief discussion of Nasser's preference for science over the liberal arts, and a fleeting reference to the state's differing methods for mobilizing students for its political purposes. There is, however, no sustained treatment of the university in the Nasser, Anwar Sadat, and Hosni Mubarak eras.

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AFRICA

EDWIN N. WILMSEN. *Land Filled with Flies: The Political Economy of the Kalahari*. Chicago: University of Chicago Press. 1989. Pp. xviii, 402. Cloth \$60.00, paper \$17.95.

When Edwin N. Wilmsen began to study San-speaking groups in the Kalahari he found that they were quite unlike the "Bushmen" of ethnographic myth. They were not isolated or "untouched." Their band structure differed sharply from the egalitarian "forager" model and they had not always been "pure" hunter-gatherers. He found, instead, that San-speakers were, and still are, an integral part of the human landscape of eastern Botswana and that, like all other peoples in the region, they have had a past of change, interaction, and adaptation, not merely a timeless antiquity.

Wilmsen began to ask why and how San reality had been transformed into "Bushman" myth. The result is a book that considers not only the processes through which San-speakers have been marginalized in the fairly recent past but also the concerns and assumptions that have led to the abstraction of these and other peoples as discrete ethnic entities available for Western study.

Wilmsen's book is about "becoming Bushmen," both in the material sense of being denied other options and thus being forced into a particular mode of subsistence, and in the cognitive sense of being categorized as a pristine "prehistoric" society. In the first chapters, Wilmsen argues cogently that previous studies have shaped the San to fit preconceived notions of how early hunters behaved and organized. In a sense, "Bushmen" provide a necessary baseline for studies of evolution as well as, for some observers, the experience of a prelapsarian culture—the original "affluent society" in Marshall Sahlins's phrase. The apparent lack of hierarchy and class structure, the context, wholly determined by ecological and ergonomic constraints, in which the San are placed, and their complete separation from neighboring peoples

and modes of subsistence are thus the product of a dominant ideology.

In reality, the San are now a dispossessed and excluded under class. In the rest of the book, Wilmsen describes a process of impoverishment that will be familiar to historians of southern Africa and brings the San back into the mainstream of history. Until the later nineteenth century, San-speaking communities played an important part in the regional economy of southwest Africa. There is now substantial archaeological and historical evidence for early cattle raising and for a flourishing and extensive long-distance trade network to which the San contributed as suppliers of ivory, ostrich plumes, cattle, and hides. As mercantile capital penetrated the Kalahari, however, first increasing the rate of accumulation and differentiation and then abruptly withdrawing, the San lost control over their land and labor and became encapsulated within the developing politics of the region whose rise was partly funded by better access to capital. They then had two options: either an independent existence as foragers (one aspect of their "traditional" image) or a dependent existence as the clients or serfs of wealthy Tswana and Herero cattle owners (the other, "parasitic," aspect). Colonial rule upheld this subordination, believing it to be traditional, and current social policy seems likely to perpetuate it.

Wilmsen's book exposes serious gaps in and misunderstandings of the history of the Kalahari region. This is important. Even more so is the way in which the author shows that these misunderstandings arise from a flawed and deficient body of theory that has in turn distorted both evidence and analysis. This opens the debate to a wider audience. The uncomfortable gap between changing social, political, and economic realities and a timeless, static ethnography is one that worries many scholars, not least because of the developmental implications; but few have been willing to confront the problem so openly. It seems that the San must be freed from ethnography as well as from poverty and it is to be hoped that others will follow Wilmsen's example.

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ROBERT B. EDGERTON. *Mau Mau: An African Crucible*. New York: Free Press of Macmillan. 1989. Pp. xii, 298. \$22.95.

This is the first general history of the Kikuyu revolt against colonial rule in Kenya to appear in over twenty years. It comes in the midst of a major reexamination of Mau Mau and its ramifications, with recent contributions by David Maughan-Brown (1985), David Gordon (1986), Tabitha Kanogo (1987), David Throup (1988), and Frank Furedi (1989), not to mention at least four articles of note in 1990 alone. Although Robert B. Edgerton shares the

prevailing conviction that Mau Mau was the crucial event in the making of modern Kenya, he bypasses many of the preoccupations of current scholarship with an approach that is anecdotal rather than analytical and addressed to a popular rather than a professional audience.

The main strength of this book is its brisk and engaging narrative. Edgerton has read widely in the literature on Mau Mau and he generally shows a sure hand in his organization and presentation of the story. His talent for extracting telling details from published works as well as from Colonial Office correspondence and private interviews is put to good use. He offers vivid vignettes that highlight the white settlers' obduracy, the forest fighters' desperation, the security forces' ruthlessness, and other elements of that tragic convergence of wills in the highlands of Kenya in the 1950s. His sympathies clearly lie with the Mau Mau activists, but they are not idealized: even the sainted Dedan Kimathi is shown as a deeply flawed figure. The settlers, in turn, are seen as all but devoid of redeeming qualities: it is their racist attitudes and narrow pursuit of self-interest that are held responsible for the conflict. The central chapters of the book are preoccupied by the sheer fury unleashed by the rebellion. Although the atrocities committed by the Kenya police, the British army, the home guard, and Mau Mau gangs deserve scrutiny, the lurid and lengthy attention devoted to them taps some of the same sensationalism that made Robert Ruark's *Something of Value* (1955) so unhappily influential in shaping the Western understanding of Mau Mau.

Edgerton's anecdotal approach tends to neglect the structural complexities of the crisis in postwar Kenya. The severe social and economic tensions operating in the white highlands, the reserves, and Nairobi receive only the most superficial treatment in his discussion of the origins of Mau Mau. He shows scanty appreciation for the diversity of groups and multiplicity of interests existing within those misleadingly simple categories of African and European (and Asian). Why the Kikuyu population divided as it did over Mau Mau—surely one of the key questions about the upheaval—receives nothing more than a passing remark or two. Apart from tales of home guard violence, little attention is paid to the so-called loyalist element of the population. Nor are the Europeans much better served. Whereas Edgerton does show that officials, soldiers, and settlers were often at odds with one another, he portrays the latter community as far more unified in its response to Mau Mau than was the case. Finally, it seems rather surprising that a professor of anthropology and psychiatry (as Edgerton is identified on the book jacket) has so little to say about the report on the psychology of Mau Mau by Dr. J. C. Carothers, which served as the linchpin for the official interpretation of Mau Mau.

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LYNN BERAT. *Walvis Bay: Decolonization and International Law*. New Haven: Yale University Press. 1990. Pp. x, 219. \$25.00.

Lynn Berat's book on the decolonization of Walvis Bay, a strategic deep-water port on the coast of Namibia, is extraordinarily timely. After decades of illegal rule and military occupation by South Africa, a long liberation war and a negotiated settlement, Namibia gained its independence in March 1990. Walvis Bay, however, remains under South African authority. It is beyond debate that "control of Walvis Bay is synonymous with control of Namibia" (p. 2). The terminus of Namibia's main rail line, Walvis Bay is the country's only deep-water port. It is the center of the fishing industry, which in terms of Namibia's foreign exchange is second only to mining. The South African enclave holds the key to Namibia's economic, and hence political, survival.

Arguing their respective cases on the basis of international law, both the South African government and SWAPO, the governing party in Namibia, have laid claim to the port. Berat assesses the validity of each position, taking into account the dynamic nature of international law. She concludes that the South African claim is rooted in theories of law that were once the norm, but subsequently have been discredited. In contrast, SWAPO's position is informed by international law as it has evolved in the post-World War II era. Berat concludes that South Africa's claim is no longer persuasive in light of the evolution of the international legal system. Rather, there is strong legal justification for the incorporation of Walvis Bay into Namibia.

Berat's conclusion is grounded in her argument that there have been three international legal orders since the late nineteenth century. Under the first, the "colonialist" legal order, Western states denied sovereignty to non-Christian and, later, "non-civilized" peoples. In the early twentieth century, the "transitional" legal order emerged, during which the rhetoric, but not the practice, of self-determination abounded. Finally, in the post-World War II era, the "modern" legal order was established. Under this regime, Berat argues, "the law of decolonization based upon self-determination and equal rights has become entrenched in the jurisprudential corpus" (p. 14). Unlike its predecessors, "the international community now includes all the world's peoples and is no longer the special domain of an exclusivist white, Western club of states" (p. 177).

Berat's overly rosy view of the present international legal order is indicative of a more fundamental flaw in her argument. She does not question the premise of international law, who makes it, and who does not. She fails to ask how peoples can have authentic self-determination when they have not been permitted to participate in the creation of the international legal systems on which their political independence is based. The Namibian people, for instance, were

granted political independence under heavily circumscribed conditions set by Western nations. The "exclusivist white, Western club of states," supposedly relegated to the dustbin of history, controlled the Namibian independence process. And it will ultimately determine whether or not Namibia gets Walvis Bay.

Although Berat does an admirable job of making her case, hers is not the most significant question. A more important project would be to assess whether or not international law is itself valid, particularly as it applies to peoples who have had no role in establishing it.

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ASIA

ARTHUR WALDRON. *The Great Wall of China: From History to Myth*. (Cambridge Studies in Chinese History, Literature and Institutions.) New York: Cambridge University Press. 1990. Pp. xiii, 296. \$39.50.

The key to Arthur Waldron's book is perhaps a brief quotation from Yang Lien-sheng (on the verso of the dedication page): "In studying the Chinese world order it is important to distinguish myth from reality whenever possible. Both can be influential." This broad hint, plus the subtitle, suggests that an "Irish bull" can serve to summarize Waldron's verdict: "The Wall's not the wall it was—but then, it never was." For the period that the book stresses (from Ming times—the great wall-building epoch and also when Europeans first glimpsed it—to the modern era), Waldron argues his case persuasively.

To underline this emphasis, the first sentence of the introduction cites Deng Xiao-ping exhorting the Chinese people in 1984 to "love our country and restore our Great Wall" (p. 1). And it was because of Waldron's interest in Ming military policy, especially this vast structure that "drained . . . the wealth of the Ming state" (p. 3) and contributed to the dynasty's collapse but which apparently had little military efficacy, that he began the lengthy research that produced this book. Meticulously footnoted, it will be an invaluable tool for scholars of the Ming and for those who merely seek to comprehend how that polity made strategic decisions and the ruthlessness (or mindlessness) with which it implemented whatever measures were approved, and punished whoever was blamed when those measures failed.

Passages in the book consider why the Great Wall served its obvious function so poorly and can furnish cautionary lessons for modern military leaders. The burden of regional requisitions provoked severe unrest in Shensi and Shansi, leading to the recall (and later execution) of the two major figures involved, Tseng Hsien, the Ordos field commander, and Hsia

Yen, the grand secretary who pushed Tseng's proposals for combining wall-strengthening and military sorties against the nomads. Tseng at least was posthumously rehabilitated. (The Chinese prefer their military heroes dead.) But the failure either to mount a preemptive strike or to finish the Wall allowed Altan Khan to ride around the uncompleted eastern end and invest Peking in 1550, a crisis that led to more wall building. The Great Wall was still far from done when the Manchus put an end to the Ming dynasty.

Waldron's book might have benefited from deeper consideration of earlier walls: city walls, those between entities in the Warring States period, and the early northern border precursors of the Wall. The book includes two photographs of the (alleged) Chao wall but makes no comment on the *Shih chi* 81 text describing Li Mu's exploits against the Hsiung-nu. Sun-tzu's advice not to besiege walled cities and Mo-tzu's techniques for defending them might also have shed light on the strategic, structural—and sentimental—problems of the Great Wall.

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MARIE-CLAIRE BERGERE. *The Golden Age of the Chinese Bourgeoisie, 1911–1937*. Translated by JANET LLOYD. (Studies in Modern Capitalism/Etudes sur le capitalisme moderne.) New York: Cambridge University Press and Maison des Sciences de l'Homme, Paris. 1989. Pp. x, 356. \$59.50.

This book, by its author's admission, is very much a product of the times. After two decades of research and writing on the subject, Marie-Claire Bergère tells us, it was the changed circumstances of the Chinese revolution that finally impressed the significance of the bourgeoisie on her thinking sufficiently to convince her of the necessity of a systematic study. For this we must be grateful. The study also reflects the imprint of the ideological predisposition of these changed circumstances, with somewhat more questionable consequences.

The circumstances are two. First, "the priority now granted to modernisation over revolution in China prompts one to reconsider the bourgeoisie's contribution to twentieth-century Chinese history," because it "was the first to identify its own destiny and that of China as a whole with economic modernisation" (p. 4). This in turn calls forth, second, a reconsideration of the relationship between state and society: the eventual inability of the bourgeoisie to assert itself was in part the responsibility of the state, which "missed its vocation to assist and encourage, as States had done in the rise of Western capitalism, or because, by default, it allowed the country to slip into the sterile evils of militarism" (p. 5).

The bourgeoisie in the study is the modern urban business class of Shanghai: product of the Treaty Port system and characterized by a commitment to operate

within the norms of modern business and conscious of itself as a new group. This class should be distinguished, therefore, from the older business class that had flourished under the gentry-bureaucratic regime of the imperial order. Following an introductory chapter that outlines the appearance of a new urban economic and social order with China's "opening" to the West in the mid-nineteenth century, Bergère devotes the substance of the study to the ascendancy of this new group during World War I and the immediate postwar years with the favorable economic and political conditions provided by the war. The study is divided into two parts. The first traces the development of the modern sector of the Chinese economy (mainly in Shanghai) and gives an account of the bourgeoisie as a social formation. The second turns to an account of the efforts of the bourgeoisie to assert itself politically during this, its "golden age." To Bergère, these efforts came to an end not with the Communist victory in 1949, but with the Kuomintang reassertion of state power after 1927, which, in reabsorbing the bourgeoisie into a bureaucratic order, extinguished its nascent creativity. An epilogue traces this latter process.

Although some account of the culture and lifestyle of the bourgeoisie would have enriched the argument significantly, the discussion of the bourgeoisie during its golden age is informative, offers a vivid portrayal of its characteristics as a social formation, and argues persuasively for the presence in China of a vital new social group straining against the limitations of its environment (and its own) to create a new economic order. Bergère successfully shows the importance of this new social formation in the social and political dynamics of urban China in the 1920s; she also draws on materials that only recently became available to give identity to its members, their visions, and their motivations.

Bergère succeeds, but perhaps too well. One problem with the study is that it claims a scope that is much larger than what it actually covers. It is in the main a study of the bourgeoisie during the half decade after World War I, but what it provides for the rest of the period it claims (1911–37) is too sparse to allow judgment over whether the bourgeoisie constituted a social formation or merely represented a conglomeration of diverse interests on which a few enlightened businessmen could impose an ephemeral (and largely illusory) unity under the peculiar conditions of the early 1920s. The problem is compounded by the absence of attention in the study to the social relations of the bourgeoisie: to other business groups with much more conventional ideas about their place in society, on the one hand, and to the emerging working class on the other, to whom the bourgeoisie did not always appear to be the enlightened vanguard of modernization that Bergère makes it out to be. There is also too little, I think, in the way of a systematic analysis of the ideology of this new group.

The major problem, however, may be the rather mechanical (and ahistorical) dissociation of revolution and modernity, which informs the argument, and an accompanying—almost compulsive—urge to prove wrong everything Marxism has had to say on the subject. This is a common trend in studies of China these days and one that, in demoting the role of social conflict and imperialism in the dynamics of Chinese society, leaves us without an explanation as to why there was a revolution in China after all. In this study, this notion appears in the form of repeated inconsistencies between evidence and inference. Thus, modernization and revolution must be distinguished, but modernization has revolutionary effects even where it is moderate, and in China it did (pp. 34–37). The "subordinate role" of China in the world economy does not explain the inability of the Chinese state to meet the challenge of modernity; but the "direct clash between unequally developed economies was no doubt important" (p. 92), and the Chinese bourgeoisie owed its golden age to the distractions of foreigners elsewhere during the war. We even learn that the effort of the bourgeoisie to assert itself politically had something to do with their being "pushed into assuming a leading political role" by American and British diplomats (p. 242).

I agree with Bergère that we need to repudiate the "dogmatic" welding together of revolution and modernity in order to see modern Chinese history in all its complexity. This study, with its recovery of the significance of the bourgeoisie, its many insights into the social and ideological dynamics of modern China, and the questions it raises concerning the relationship between state and society, makes an important contribution to this end. At the same time, it also cautions us at a critical juncture in the historiography of China against the substitution of one dogma (modernity) against another (revolution) that makes a caricature of the Marxist contribution to understanding problems of development in a less-developed society, and reduces the concept of modernity to developmentalism, ignoring that revolution itself was a product of modernity, as much of its evidence suggests.

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CHARLES W. HAYFORD. *To the People: James Yen and Village China*. (The U.S. and Pacific Asia: Studies in Social, Economic, and Political Interaction.) New York: Columbia University Press. 1990. Pp. xviii, 304. \$35.00.

It is a pleasure to review a book that not only has a persuasive thesis but also is gracefully written. Such is the case with Charles W. Hayford's study of James Yen and village China. Hayford details the arduous transformation of what was initially an elitist, urban approach to aiding the poor by teaching them to

read. Yen's associations with the Young Men's Christian Association (YMCA) and other Christian organizations were close, and his strategy evolved out of their middle-class, individualistic, and self-help orientation with its implicit westernization. Although Yen did not consider the Mass Education Movement a Christian organization, he consistently maintained that he was implementing "Christian ethics," and he learned much from the public campaigns and socialization techniques of the YMCA.

Yen and his colleagues, however, gradually came to realize that village reconstruction would have to do more than bring literacy; it would necessitate simultaneous changes in land tenure and the economy, in the power structure, in health facilities, and in the social organization of rural China. Through trial and error and conflict and opposition, members of the Mass Education Movement learned that the peasants must become involved in and committed to the reforms. Village administration and educational programs must be "deprofessionalized" and must include the peasants themselves. Villagers must share in the financial support and in policy decisions. Otherwise, changes would be resisted, undercut, and often abandoned after departure of the external worker.

Hayford presents the Mass Education Movement as a liberal alternative to the authoritarian tactics of Maoism in village transformation. Overtaken by the Sino-Japanese and civil wars, the Mass Education Movement never had a fair trial, although it left a legacy in the Joint Commission on Rural Reconstruction, which would transfer to Taiwan, and in village reform programs in Southeast Asia. One could question whether the Mass Education Movement was a viable alternative to Maoism and whether it ever had the possibility of becoming a nationwide campaign even without the tragedies of war. Implementation outside China came only under special circumstances. Although acknowledging differences in ideology, Hayford, nevertheless, makes a convincing case for continuities between the mass campaign tactics and the populism of the Mass Education Movement and of Maoism: "The techniques of the literacy campaign [had] spread nationwide and [had become] a permanent part of China's adult education repertoire" (p. 46).

This presentation of the variety and complexity of reform movements in republican China is instructive. A rich historical context and apt metaphors and comparisons lend vividness to "the ambivalent and sometimes tortured attempts of educated Chinese to understand and uplift their country cousins" (p. ix). The research base is strong and includes extensive interviews with Yen and other participants as well as consultation of contemporaneous writings.

I have two minor quibbles: the organization of the book leads to some repetition, and the bibliography is so selective that abbreviated footnote references to works not in the bibliography have to be traced back

to the first footnote citation. Nevertheless, it is an important work and is also good reading.

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JOHN W. GARVER. *Chinese-Soviet Relations, 1937-1945: The Diplomacy of Chinese Nationalism*. New York: Oxford University Press. 1988. Pp. viii, 301. \$34.50.

Although John W. Garver is to be commended for his resourcefulness and diligence, his suggested focus on a "fusion" of nationalism and communism in China as embodied in this book's title, and his straightforward designation of Mao Zedong as the author of such a successful amalgamation, has created questions that require his response. With rich information at Garver's disposal, the reader wonders whether his interpretation is consistent with the facts that he provides.

Garver must be aware of Mao's public representation of himself as Joseph Stalin's disciple. Mao, in fact, successfully implemented Stalin's revolutionary strategy in China, demonstrated by leadership of the proletariat and its party, alliance with the peasantry, "armed revolution against armed counter-revolution," the united front, and the Soviet form of government. Even after the successful revolutionary seizure of power, Mao served Stalin's purpose by entering into the Korean War on the Soviet leader's behalf. Thus, under the banner of China's national interest, the Chinese Communist party is only to mobilize and use nationalism to serve international Communist movements as any of the Communist national detachments or parties should.

China, like many other underdeveloped and oppressed nations, must, and is in the most convenient position to, champion national survival. The immediate task of Communist parties of the Third World countries is to aspire to national independence and equality, not only for immediate national salvation but also for the international struggle of the Communist movement. Thus nationalism, for the Communists, should serve only as a vehicle for their far-reaching proletarian internationalism or international communism. For the Chinese Communists, nationalism is only the means, but communism is the end. The Chinese Communists would indeed be amused if they were presented merely as nationalists; they might also resent such a description and consider it humiliating to underestimate their global aspirations. The book's title, particularly the subtitle, is not in harmony with the subject matter since Garver means to discuss primarily the triangular interaction of the forces of the Soviet Union, the Chinese National Government, and the Chinese Communist party.

In discussing the triangular relationship among the Soviet Union, the Chinese Nationalists, and the Chinese Communists during China's War of Resistance

against Japan, 1937–45, Garver rightly commends Chiang Kai-shek for his unusually fruitful policy toward the Soviet Union. He recognizes that Chiang secured substantial Soviet support at a time when the United States was isolationist in policy orientation and other major powers such as Great Britain and Nazi Germany were preoccupied elsewhere. Garver points out that, although Chiang did not secure direct Soviet participation in the war effort until Japan's virtual collapse at the time of the atomic blasts of Hiroshima and Nagasaki, he did retain basic Soviet moral support. In addition, Chiang enabled Governor Sheng Shih-tsai's defection after a decade of subservience to the Soviet Union, thus causing the Soviet Union to withdraw gracefully its influence from Xinjiang.

Garver congratulates Chiang's efforts in leading China "to victory over Japan against all odds" and suggests that Chiang succeeded in obtaining Soviet acceptance of China's rise to great power status. Certainly, China's founding membership in the United Nations (UN) and permanent membership in the UN Security Council can be cited as concrete proof. Garver must also agree that Chiang even succeeded in securing, nominally at least, a Soviet pledge under the 1945 Sino-Soviet Treaty of Alliance to direct all Soviet aid to China exclusively through his National Government, thus depriving the USSR and the Chinese Communists of any legal basis for direct contact with one another.

Interestingly, Garver states that Mao's "greatest achievement . . . in his emancipation of the Chinese Communist Party from Moscow's domination . . . was not an easy accomplishment." Later in his leadership, Mao did earn certain dignity for the Chinese Communist party as compared to other parties, but it was based on mutual respect and teamwork rather than a struggle for emancipation against domination. Similarly, for the sake of communism, Mao even challenged Nikita Khrushchev on behalf of the late Stalin. On the whole, Garver's book is provocative and rich in documentation of Western and Chinese language sources, but regrettably it contains practically no Russian language sources.

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JAMES R. RUSH. *Opium to Java: Revenue Farming and Chinese Enterprise in Colonial Indonesia, 1860–1910*. (Asia East by South.) Ithaca: Cornell University Press. 1990. Pp. x, 281. \$34.95.

The opium "farm" was a monopoly concession for selling opium in colonial Java, sold by European administrators to Chinese merchants in spectacular annual auctions. In addition to paying dearly for exclusive marketing rights in one of the island's twenty-one provinces, the Chinese who dominated the trade agreed to purchase only government sup-

plies of opium. In the mid-nineteenth century, income from these arrangements represented 17 percent of all of the revenues of the Indies and covered 32 percent of the costs of local administration. Less formally, the trade also provided Javanese and European administrators with generous supplements to their official income, as Chinese merchants lavished them with gifts of goods and money. Opium underwrote the wealth of Java's richest Chinese, created a vast network linking Chinese traders to rural society, and made addicts (most of them only mildly dependent, since they were too poor to afford larger doses of the drug) of at least one out of every twenty Javanese. In short, James R. Rush shows us that the opium farm was "one of the distinctive institutions of colonial Java" (p. 1).

Rush's study is the first definitive history of the opium farm system. Although Rush concentrates on the period of the system's mature development, from 1860 to 1910, he also describes the role of opium from the first years of Dutch colonialism in the 1600s through Indonesian independence in 1945. By linking his analysis to this larger context, Rush explains the history of opium and much more in colonial Java.

Rush demonstrates, for example, that despite perennial expressions of concern for native welfare, colonial officers eagerly promoted opium use. It was just too lucrative a source of revenue. Smuggling provided the island with half of its opium supply, but the government's intermittent antismuggling campaigns were primarily intended to defend state monopolies, not reduce use. The most important players in the trade, however, were elite *peranakan* (Indonesianized) Chinese. Standing atop a vast network of stores, carriers, opium dens, brothels, and rural "spies" (who insured that competitors did not market their opium in a farmer's district), these elite Chinese amassed enormous fortunes and developed powerful patronage organizations. Despite harsh European restrictions on Chinese travel, these traders used their opium networks to penetrate the countryside and dominate rural trade and moneylending. Meanwhile, native Javanese were little more than passive consumers of opium, although some profited by selling the product in a black market that reached even into the food stalls of remote villages.

Far beyond the details of the opium trade, there is much in this book relevant to a reconceptualization of colonialism in the Indies: the ethics of European administrators, the internal organization of Chinese firms, the complicity of Javanese aristocrats in the exploitation of the native population, and the evolution of the colonial state into a modern centralized bureaucracy. The author's painstaking account provides insight into one of the colonial era's most unseemly institutions and an optic through which to trace the development of state and society in the colonial era as a whole. This work deserves to be read by all students of Indonesian and Asian history as it

brilliantly challenges received understandings of colonialism and economic development.

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MICHAEL LEWIS. *Rioters and Citizens: Mass Protest in Imperial Japan*. Berkeley and Los Angeles: University of California Press. 1990. Pp. xxiv, 314. \$42.00.

During the final months of World War I, so-called rice riots broke out throughout rural and urban Japan. Inflation combined with market speculation, hoarding, and cornering manipulations to push the price of rice beyond the purchasing power of millions of Japanese citizens. Met initially by government indifference and inaction, citizens rallied in some regions to force merchants to lower prices and in other regions to demand that government take remedial steps to ensure that rice was sold at reasonable prices. Between July and September 1918, forty-two of Japan's forty-seven prefectures reported incidents of rice riots, many of them violent and, all told, involving over a million people. The military had to be mobilized in twenty-six prefectures. Some twenty thousand citizens were illegally detained, over eight thousand were prosecuted for rioting, and more than thirty protesters were killed. As the "riots" ended, the last of Japan's "transcendental cabinets" fell from power, ushering in the first of several party governments in what became known as Taisho Democracy.

Michael Lewis's finely researched book is the first full-length account in English of these events and should, for this reason alone, be widely welcomed. Until now American scholars studying the Taisho era (1912–25) have seldom devoted more than a brief paragraph to the rice riots, dismissing them as little more than epiphenomena indicating growing popular discontent but not signaling more basic structural changes at work.

For Lewis the riots represent much more. They show a "massive rejection of the new Japan" (p. 84), by which he means the self-consciously modernizing Japan of the Meiji era (1868–1912), which forbade popular participation in the political process. Lewis amply demonstrates that the riots were not monolithic but encompassed traditional forms of rural peasant protest as well as modern urban labor demonstrations and strikes whose goal was nothing less than gaining greater political representation within the existing constitutional order. Thus, the riots serve as "tracers" of rural-to-urban demographic shifts, the addition of a million new workers to the labor force since the beginning of World War I, the growth of a national market, the social effects of greater inequality of income, the rise of a new middle class and *nouveau riche*, and so on.

The motivation of the rioters differed according to the larger social context in which they protested high prices. In the rural areas rioters acted according to

the unspoken premises of the "moral economy," whose tradition included a "right to survive" and strong communal notions about right and wrong. In urban areas and in the mining towns, a "market rationality" favoring better wages and working conditions informed the protesters' demands; urban rioters more explicitly demanded greater democratic rights and fairer distribution of wealth and political power. In showing these differences, Lewis reveals that Japan in 1918 was still very much a transitional society in which traditional and modern forces could react to paternal government in similar fashion but have very different goals.

Lewis succeeds in giving voice to the "otherwise inarticulate sections of society" (p. xviii), but he gives them both too little credit and too much credit for their actions. On the one hand, he demeans their protest by misnaming their actions as "riots" (*sōdō*), the Taisho government's biased designation; on the other hand, he inflates the consequences of their protest by asserting that "the riots altered the course of Japanese history" (p. xix).

Lewis is careful to show that, regardless of region, most protesters acted with amazing moderation, usually relying on nonviolent protest in attempting to effect reforms (pp. 16, 47, 51, 59, 63, 87, 99, 149, 155–56, 222). Even when their behavior turned violent, "rioting" was not random or always "spontaneous," and not even very destructive, but was governed by "unwritten codes" or "rules" and, as often as not, was a response to police repression. Lewis himself points out that "riot" is a misnomer in a great many instances; hence, it is all the more curious that he employs the term with silly consistency. Even conceding that to place "riot" repeatedly within quotation marks would have been tiresome for both the reader and the writer, nonetheless Lewis's persistent use of the term misleads and even misinforms.

It is also dubious whether the riots were the historical turning point that Lewis argues. He recognizes that the riots were merely "symptoms of deeper changes" yet is not shy about alleging that "the protests resulted in far more than just the replacement of [General] Terauchi by a 'commoner' prime minister" [my emphasis] (p. 244). Lewis is correct in saying that new government regulations on the sale and distribution of rice were a direct result of the riots, yet he is on much shakier ground in suggesting a causal relationship between the riots and the rise of popular activism, labor union organizing, tenant union agitation, antimilitarism, and the short-lived flowering of Taisho Democracy. The riots may have "significantly accelerated the pace of political and social change" (p. 252) in the 1920s, but so, too, might have the influence of the Russian revolution, Wilsonian liberalism, Christian socialism in Japan, as well as labor and farmer movements that antedate the rice riots.

Lewis's most valuable contribution is his convincing evidence showing that Japanese citizens were not

averse to relying on protest in redressing grievances whose root cause was unrepresentative government—in short, in showing citizens as “rioters.”

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LAURENCE W. PRESTON. *The Devs of Cincvad: A Lineage and the State in Maharashtra*. (Cambridge South Asian Studies, number 41.) New York: Cambridge University Press. Pp. xi, 273. \$49.50.

This is a fascinating, powerful, yet curiously incomplete book: fascinating because of the wealth of detail and powerful because it demonstrates that case studies of indigenous structures and institutions are essential to understanding socioeconomic change in India. Laurence W. Preston focuses on the Devs, a Desastha Brahman lineage with *inamdar* (revenue-collecting) rights to some twenty-two villages. The Devs of Cincvad also had other privileges not connected with religion. The British scrutinized these privileges (granted by Shahu and the Peshvas, heads of the Maratha state, from the seventeenth century on) after their conquest of the Deccan in 1817–18. They found the Maratha archives invaluable for their investigation of claims on the state revenues.

Preston uses Marathi and English archival sources in Pune and Bombay to see how the Dev lineage fared in the transition from indigenous to foreign rule, how it adapted to the British government when questions of inheritance and status arose (p. 5–6). His painstaking use of, indeed reliance on, those rich sources is the book's greatest strength, yet, in my view, problems with the sources make the book incomplete. It is liberally sprinkled with indigenous words, but there is no note on transliteration or glossary, and these words are not all in the index. Nowhere does Preston comment on the state of the records in either Pune or Bombay or the extent to which he was able to use them. He hints at problems (for example, p. 29 and p. 82), and other scholars would have benefited from even minimal comments (perhaps the press limited him here). But the most striking omission is that Preston does not indicate whether there are still Devs of Cincvad and, if any descendants of the lineage are alive (which seems likely), why he did not think it worthwhile to contact them.

As I and other authors of books that Preston cites can testify, descendants can furnish documents and stories that complement and extend archival sources. Preston indicates that, because of his sources, individual Devs recede behind the corporate entity (p. 12). But in his later chapters individuals are significant, and Preston speculates on their motives and strategies. Omission of family-held and oral materials may affect some of his arguments. He wants to examine the internal history of the lineage, but his sources are

those that the lineage presented to the governments. Those who ceased to be represented in lineage records might have challenged his view that corporate unity was ultimately preserved.

There are other questions of interpretation, most of them possible only because of the rich material provided by the author. Nose down in the records, Preston's view of the larger picture sometimes falters. He refers to the Maratha archives as “unique” (p. 245) and speaks of the granting of numerous small cash allowances as a characteristic feature of the “Hindu state” (p. 92). Yet the Nizam's Hyderabad State archives are obviously very similar and the wording might better have been “Indian state.” Preston faults the British for getting enmeshed in inheritance regulation within the lineage, but surely that was what Shahu and the Peshva did when they resolved disputes between the Devs' kinfolk and what the Mughals and Marathas routinely did when confirming succession to privileges. Finally, Preston wants to understand “indigenous categories of social organization” (p. 9), which is not really part of his agenda, and his intermittent efforts lead him into difficulties. Although he generally sees the Devs as acquiring knowledge of the British administration and strategically adapting their presentation of cases, he also says that “the petition to Bombay was a chance for the *inamdar* to express his own understanding of his rights” (p. 189). While he says “we should not doubt the sincerity of the Devs when they described their inheritance as *devasthan*” (granted to a deity, not a person), he immediately labels a widow's appeal for that status for the estate as Machiavelian (p. 219). Later he imposes orthodox brahmanical inheritance doctrine on his lineage, despite his own evidence and recognition of the considerable “pragmatism” influencing its inheritance practices (p. 242). And rather than use indigenous terms, Preston borrows from Benjamin Disraeli the concepts of “rights” and “authority” for his final analysis: to safeguard their rights, *inamdars* and other privileged subjects must maintain corporate unity and, to maintain authority, the state must pay close attention to the limits of authority, particularly where the regulation of inheritance is concerned (pp. 238–39). Still attuned to his sources, Preston focuses in the final pages on the failures of the Inam Commission rather than on the internal history of the Devs (pp. 245–47).

Overall, this is a fine book, the result of long, hard work, and it makes significant contributions to our knowledge and interpretation of the eighteenth- and nineteenth-century history of India.

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DESLEY DEACON. *Managing Gender: The State, the New Middle Class, and Women Workers, 1830–1930*. New York: Oxford University Press. 1989. Pp. x, 308.

The specific subject of Desley Deacon's book is the complex relationship between the development and professionalization of an Australian public service bureaucracy and the places allotted to women within this bureaucratized public service. At this level, the work is a meticulous study of local employment practices and policies, battles over national legislation, and the uneven effects of such ideological constructs as the family wage or scientific motherhood. The larger subject of Deacon's book is the role played by gender in the emergence and consolidation of what she calls the new middle class. This class, which stands between the bourgeoisie and the proletariat, consists of workers who derive their income not from owning the means of production or from manual labor but from the sale of educational, technical, or social skills. According to Deacon, "gender divisions constitute a major fault line within the new middle class today" (p. 11). The gendered nature of the class's internal divisions—between managers and routine clerical or semiprofessionalized workers—means that women and men who occupy the clerical or semiprofessional, feminized positions earn less money and have less mobility than the men and women who occupy the more elite, masculinized positions. At this more theoretical level, Deacon provides one historically and geographically specific case study of the emergence of this "fault line," as well as a convincing argument for the necessity of examining gender in any analysis of state or class formation.

The century that Deacon examines, 1830–1930, cannot be plotted neatly as a tragedy or comedy for women's employment opportunities, and one of the many virtues of this study is Deacon's attentiveness to the way that other developments in national and international economics and politics affected women's employment in various parts of Australia. Thus, the relatively decentralized and underpopulated Australia of the mid-nineteenth century saw many women installed as postmistresses, especially in provincial districts. With the economic downturn of the mid-1880s, however, members of the new middle class acted to protect male employment, in the name of the family wage, by restricting the labor of married women in particular. Between 1896 and 1900, single women briefly enjoyed more employment and wage parity, partly through the mastery of typewriting and shorthand skills, but after 1900, male capitalists and trade unionists combined to curtail women's work again. Throughout this study, Deacon examines the ideological norms of masculinity and femininity that underwrote and were constructed by these developments. She is particularly astute on the way that individuals' class positions and personal biographies shaped their contributions to these material and ideological effects.

This book provides a subtle and appropriately complex model of historiographical interpretation. Although she ultimately privileges class over gender as an explanatory category, Deacon is intent on

demonstrating that these determinants never exist in isolation from each other. Her skillful transitions from individual biography to local history to international context reveal the layers of narrative she constructs throughout this work. Deacon's ability to move from historical detail to theoretical generalization makes her book a model of the amalgamation of history and theory that many historians are struggling to forge.

MARY POOVEY
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I. C. CAMPBELL. *A History of the Pacific Islands*. Berkeley and Los Angeles: University of California Press. 1989. Pp. 239. Cloth \$30.00, paper \$10.95.

The Pacific Islands region is not taken seriously by modern historians or, for that matter, by anyone else. The islands and their peoples have always been relegated to a second-class status in world affairs, a place to be traversed or flown over to go elsewhere.

Such a generalized attitude toward the island areas and nations has been established over a long time beginning with the European explorers of the seventeenth and eighteenth centuries. This attitude exacts a price; not much deliberate, synoptic history has been written about the islands, and that which has tends to be overused and not up to the standards that would apply to historical writing on the developed countries of the world. Moreover, most of the histories are written by nonislanders who do not live in the region. Historian Greg Denning has correctly referred to the Pacific as "an historically underdeveloped place."

I. C. Campbell attempts to lead historians out of this mediocrity by presenting what he claims is the first history of the region to be published since 1951. He succeeds to some extent, although the book is too short to do justice to a comprehensive consideration of the Pacific Islands. Campbell devotes some thirty pages to the pre-European discovery period and some two hundred to the subsequent era. This, of course, is acceptably congruent because more is known about the islands and their peoples after European discovery than before, and Campbell makes his precontact historical points quite well.

The twentieth-century concept portraying colonialism as a moral evil that should be eradicated represented a response to the colonial-imperial situation in the Pacific since 1700. The Pacific Islands, which Europeans colonized between the sixteenth and nineteenth centuries, gained self-government or independence by the end of World War II. Campbell treats this issue with a circumspection that might disappoint some islander intellectuals and nationalists. Moreover, Micronesia receives only superficial attention, and Japan—one of the important Pacific colonial powers—is, surprisingly, omitted entirely.

Over the past fifteen years in the northern and

western Pacific Islands region, the emerging governments have begun to drop their old colonial period place-names and returned to common usage in their traditional or ethnic forms. Hence, Kosrae replaced Kusaie, Pohnpei replaced Ponape, and Chuuk replaced Truk, to name only a few. Campbell has made note of some of these changes in his maps, but not in his text, another offense against the indigenes.

Because Campbell undoubtedly intends his book to be an introductory overview of the Pacific, he has accounted for his sources not with footnotes or endnotes but by organizing his recommendations for further reading, by chapter, at the end of his narrative. This is sound and certainly convenient, but there might have been more weight here; some of the chapters' sources are too sparse.

Although I have criticized Campbell's shortcomings in the present volume, I nevertheless recommend his book. History professors and teachers in the western metropolitan countries will find it a useful and readable overview on the undergraduate levels.

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UNITED STATES

HELENA M. WALL. *Fierce Communion: Family and Community in Early America*. (Harvard Historical Studies.) Cambridge: Harvard University Press. 1990. Pp. x, 243. \$29.95.

This book captures the energy exerted by communities to control the private lives of their citizens in each of the North American colonies during the first century of settlement. Helena M. Wall is aware that it is potentially dangerous to meld North to South and to describe all communities without any special attention to geographic location or religious persuasion. She argues persuasively, however, that in matters of family and community, colonists everywhere were more similar than different. Communities were pre-eminent, the family was the source of social order and values, and stability could be protected by granting officials everywhere the right to pay "close attention to individual conduct and morality" (p. 4).

Wall relies almost exclusively on court records that, she believes, reveal both the nature of conflict as well as the discourse of local life. Courtrooms were the stages on which the community publicly dramatized its grievances against its citizens for breeches of conduct that threatened harmony. Clearly, New England Puritans set "standards more stringently and enforced them more zealously" than other colonists, but even in the Chesapeake, where control was more relaxed, "personal conduct became a matter of public concern" (p. 4). Neighbors were expected to report any infractions of the rules and they protected personal reputations with a seemingly endless array of cases of slander. Communities avidly enforced spousal responsibility in marriage, and the raising of

children was a communal task. Wall suggests that the "lines between family and community were so faint and so often crossed as to be almost meaningless" (p. 144).

In the afterword, Wall synthesizes the scholarship on demography, family life, women, and community to account for the changing character of the family. Many impulses contributed to the redefinition of private life, resulting in families that were "affectionate, voluntaristic, and private" (p. 138). The family became a refuge from the outside world and, by the early nineteenth century, families emerged as separate from the rest of society: men left home to work and women turned their full attention to the household.

I like very much Wall's argument that early Americans everywhere shared notions about the relationship between families and communities. Wall's anecdotal approach, however, is problematic. Most paragraphs are constructed with noncontextualized examples from colonial courts. As a result, the nuances are masked and some colonies, such as Pennsylvania, receive short shrift. Perhaps more serious, the concluding analysis relies on an uncritical reading of the existing literature to trace the emergence of the modern American family. Should we, for example, continue to use the Republican Mother as a model to describe all late-eighteenth-century women? Too little space is devoted to an analysis of how the conflicts between families and communities evolved and whether in the later period differences based on geographic location and religious impulses can continue to be ignored. Moreover, Wall describes only a limited segment of the early American population because she is unconcerned with issues of ethnicity and class. Nonetheless, *Fierce Communion* provides a useful beginning and a suggestive framework in which to study the relationship of families and communities in early America, and it reveals the fruitfulness of an approach that searches for similarities rather than differences among colonies.

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J. WILLIAM FROST. *A Perfect Freedom: Religious Liberty in Pennsylvania*. (Cambridge Studies in Religion and American Public Life.) New York: Cambridge University Press. 1990. Pp. x, 221. \$42.50.

Given the importance of the Pennsylvania story to the development of religious liberty in America, it is surprising—as J. William Frost notes—that this story has received so little attention. Here, at last, we have the vital narrative offered in impressive and illuminating detail. Many might have shied away from doing what Frost has undertaken for at least two reasons. First, the religious history of Pennsylvania is quite complex, preventing the kind of easy generali-

zations that might be offered, for example, about early Connecticut or colonial Virginia. Second, religious liberty itself is more ambiguous in Pennsylvania, lacking not only the clear status of establishment but also the clear church-state separation of a Roger Williams or, later, a James Madison or Thomas Jefferson. The story requires, therefore, a delicate touch, with nuances, qualifications, and careful discriminations.

In treating first the early period of Quaker domination, Frost acknowledges that William Penn himself did not consider all the implications of a full legal equality of the many churches and sects that flocked to his haven. But Penn and his followers did see Quakers as being in a unique position in "their" colony, determining "the laws and government and the tone of the society. Others would be welcome but they would have to be co-governed by Quaker principles" (p. 18). Within a couple of decades of the colony's founding, this simple dream collapsed in the struggle with Anglicans who, although not numerous, had strong political and ecclesiastical ties, as well as even stronger pretensions concerning the proper place of England's church in England's colonies. But Anglicans could not secure their legal establishment, either, so that these two groups jockeyed for position for a quarter of a century or more.

By the middle of the eighteenth century, many other religious voices were being heard as Quakers lost their majority control of the assembly. Although a "Quaker party" (as opposed to a Proprietary party) continued to be a factor in political alignments, the Quaker party included many who were not Quakers and not necessarily pacifists. By the time of the revolution, the Quaker party opposed the Presbyterian party, with Quakers resisting and Presbyterians strongly supporting it. And, in the midst of revolution, the new state constitution promised religious liberty but limited office holding to those who believed in God and affirmed the divine authority of the Old and New testaments. Frost labels what survived by the end of the eighteenth century a "republican religious liberty," that is, an absence of coercion in religion joined with a prevailing assumption that a republican form of government required a virtuous citizenry, which required moral instruction, which required religion (pp. 77, 84, 106). To be sure, the ecclesiastical and civil estates were separate, but erecting a wall between them seemed unnecessary at best, dangerous at worst.

Frost describes the battles in the nineteenth century within Presbyterianism, the rapid growth of Roman Catholicism, the problems of religion in the public schools, and the crazy-quilt pattern of Sunday laws. In the twentieth century, Pennsylvania has been responsible for many major church-state cases decided by the Supreme Court, and Frost is not overly pleased with the decisions of the high tribunal. He would prefer more reliance on natural law along with voices of politicians, clergy, and the citizenry at large. He

would, in fact, prefer a more accommodationist stance, which in his view is more faithful to Pennsylvania's long and convoluted history. The current Court is likely to make him somewhat happier.

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DONNA MERWICK. *Possessing Albany, 1630–1710: The Dutch and English Experiences*. New York: Cambridge University Press. 1990. Pp. xii, 312. \$44.50.

Of all the first commercial centers Europeans settled in what became the United States, only Albany stood inland and upriver. In a probing, original, but sometimes disconcertingly disjointed book, Donna Merwick grounds that uniqueness in the town's Dutch origins. Its settlers, she argues, defined themselves and their surroundings very differently from the English conquerors who eventually followed them. They sought lives that would be fully *burgerlyk*—mercantile, amphibious, and republican—rather than agrarian, rooted, and subservient to authority, in the English manner. Employing the vocabulary and insights of cultural anthropology, Merwick presents the town's early years through a succession of tableaux—of colonizing, trading, experiencing conquest, and adjusting to it—constructed from the extensive written and cartographical records left by contemporaries. To write Albany's history, she believes, requires an uncovering of the layers of socially constructed meaning laid down over time and expressed, often unconsciously, in landscapes, artifacts, and symbol systems. Full possession of the land came from controlling its interpretation.

There is much of interest in the book. In the company of such scholars as Alice Kenney and Oliver Rink, Merwick is surely right to insist that New Amsterdam's history has been obscured by New York's. We must recognize the cultural consequences of the New World conquest of Europeans by other Europeans. Among the book's collection of tableaux, its "braided knots," those reconstructing the town's *handelstijd*, or trading season, and documenting the imposition of English ways of government and ownership are especially valuable. Merwick's command of Dutch materials provides illuminating parallels drawn from life in the Netherlands and the European colonization of South Africa.

Yet the book is eccentric as well as original. At a time when scholars are documenting the strength and diversity of regional cultures on both sides of the Atlantic, Merwick's insistence on the cultural divide separating all Dutch from all English is too arbitrary and neglects the cultural brokers who moved within and between the different nationalities. Her pages on Leisler's Rebellion, for instance, cannot account for regional rivalries, for the divisions within Dutch Albany, or for an English Jacob Milbourne leading

Leisler's forces against a Pieter Schuyler upholding ties with the English regime. Moreover, her landscape is one that all but ignores the continuous significance of Albany's strategic role and its relations with the surrounding Native Americans. Instead, it is the Dutch who emerge as the land's aboriginals menaced by martial and arbitrary invaders, the English. At this point, too, Merwick's evident distaste for the latter's role—mirrored in her unwillingness to pay even passing acknowledgment to most recent studies of English New York—plays her false. It seeds her work with a surprising number of minor factual errors on English matters; more seriously, it prevents her from perceiving aspects of New England society (such as the picture drawn by Stephen Innes of a highly commercialized Springfield alongside Albany) that contradict her depiction of English ways. The book should be welcomed as a stimulating account of the transmission and reaffirmation of cultural values in America; it should not be taken as the balanced and comprehensive local history that its title would suggest.

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MICHAEL DUREY. *"With the Hammer of Truth": James Thomson Callender and America's Early National Heroes*. Charlottesville: University Press of Virginia. 1990. Pp. viii, 225. \$28.50.

Michael Durey examines James Thomson Callender's ideas, life, and journalism to ascertain whether this most despised of early American immigrant journalists deserves the place accorded him in the historical record. He concludes that Callender deserves better. Finding that historians' treatments of Callender conform to statements by his enemies, Durey's purpose is clear: to subject the actor to fairer assessment than that based on uncritically accepted "partisan assertions." The book presents a sensitive probing for alternate interpretations with thoroughgoing analysis that is uncompromising, despite revelations that evoke sympathy for one known for despicable behavior. The careful examination holds these assertions up to microscopic scrutiny and rigorous analysis, offering evidence to validate some, invalidate some, and to support both traditional and alternate interpretations of others.

Two skillfully integrated themes flow throughout. One follows Callender within the context of the human condition, exploring his formative years and the tempering across time of illusions and aspirations through trials of the spirit of a very spirited individual. The other follows the milieu in which Callender sought to realize the efficacy of his purest, most intense beliefs that, although put to work enthusiastically by himself and others on behalf of partisan interests, transcended party ideologies. His beliefs and tenacity could wreak havoc in a highly charged

political atmosphere when expressed without regard to partisan interests or ideological shifts. And they did, according to this analysis. Given the same conditions, he could also become a pawn at the behest of those more powerful. And he did, as portrayed here. Jefferson, Hamilton, John Adams, and other victims of Callender's pen are presented as less than innocent in that victimization.

Within such an analysis, Callender's disillusionment and ultimate reckless vengeance become not only understandable but predictable. Repeatedly experiencing betrayal at critical points in his life, Callender grew increasingly unforgiving and perceived Jefferson's indifference following Callender's imprisonment and devastating personal struggles in behalf of republicanism as the ultimate betrayal.

Although there is room for further analysis of Jefferson's behavior regarding Callender, the book contributes fresh perspectives and insights about their relationship and about a range of issues in the late eighteenth century. The three chapters about Callender's pre-America years provide intriguing insight about Scottish and English affairs. Of special interest generally, and to journalism historians in particular, are Callender's views about governmental organization and functions and about the press and its role in the American context.

But the book's foremost contribution is its reconsideration of the record, specifically its emphasis on Callender for what he believed, wrote, sought, and attempted, not for his castigations of the powerful. Durey notes a two-part legacy, arguing that Callender's primary importance lies "in his awareness of the potential contradictions within a political society which, founded on the doctrine of popular sovereignty, was only slowly coming to terms with that doctrine's implications" (p. 174), and that he forced "a reconsideration of what limits should be put on freedom of the press" (p. 175).

Beyond reconsidering the record about one journalist, the book contributes to our understanding of the period by redirecting attention regarding the era of Callender's life. Significantly, it should prompt a recasting of research questions regarding journalism, partisanship, and individual motives and political maneuverings in the early years of the nation.

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JEFFERY A. SMITH. *Franklin and Bache: Envisioning the Enlightened Republic*. New York: Oxford University Press. 1990. Pp. 222. \$28.00.

This work may be thought of as a sequel to Jeffrey A. Smith's recent *Printers and Press Freedom: The Ideology of Early American Journalism* (1988). Both books identify the colonial press with the reformist Enlightenment-Whig tradition, and they share a Jeffersonian

bias. The more recent work is an uncritical—almost filiopietistic—celebration of the author's two heroes in the light of their alleged selfless idealistic philosophy. Their enemies would fix on the new country institutions designed to protect and enhance the property of the rich and the well-born. To the contrary, Benjamin Franklin, Benjamin Franklin Bache, Thomas Jefferson, Tom Paine, and others of their party speak for the people as against Alexander Hamilton, John Adams, and others who are the people's oppressors.

It is admittedly refreshing to have a sympathetic assessment of the usually historian-bashed Bache. It was the times, not journalistic intemperance, that pushed him from an alleged Franklinian coolness to the kind of calumny for which he became infamous. "Virtue," writes Smith in a key sentence, "became less a matter of classical republican self-control and more a matter of Enlightenment natural rights and self-fulfillment" (p. 21). Here lies the explanation, according to Smith, of the grandson's post "self-control" inflammatory rhetoric. But in light of Smith's own evidence of the *Philadelphia Aurora's* constant financial problems, it is difficult to believe that Bache had the respect of many of his contemporaries. Furthermore, a perusal of J. A. Leo Lemay's *The Canon of Benjamin Franklin, 1722–1766* (1986) would indicate that the older man was at times no less heated, only more artful, than his grandson.

Even so, the Bache portion of the book, unfortunately the lesser portion, will prove the most useful to scholars. We learn of the young man's highly unusual education in Paris and Geneva, and how he returned to America thoroughly impregnated with his grandfather's radical principles. Inevitably, Benny Bache entered the older man's profession of printer/publisher. "Consistent with much of what Franklin taught," Smith writes, "Bache's idea of public duty was an egalitarian urge to protect the general public from the prosperous and powerful" (p. 135). Young "Lightning Rod," therefore, rapidly became the leading publicist for the emergent Jeffersonian party.

Bache's travails, financial and physical (he was maltreated by mobs irate at his denunciations of Washington), are all too briefly discussed in what is nevertheless the most interesting part of the book. The turning point for Bache was the Federalists' acceptance of Jay's Treaty. Bache's paper warned that the people should "be lulled no longer into a dangerous security by the arts of those whose interest it is to deceive them" (p. 139). The paper's attacks on Washington were "designed to destroy the president's name" (p. 139) and thereby weaken the Federalists' hold on the public.

During the administration of Adams, whom Bache likewise treated with vitriol, the Federalists finally came up with the Sedition Act, with Bache's *Aurora*, we learn, as a principal target. It was during the ensuing political turmoil that Bache, at twenty-nine the father of four boys, died during an epidemic of

yellow fever. Ironically, four days later his arch-enemy, John Fenno, editor of the *Federalist Gazette of the United States*, followed Bache to the grave.

THOMAS WENDEL,
EMERITUS
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SHEILA L. SKEMP. *William Franklin: Son of a Patriot, Servant of a King*. New York: Oxford University Press. 1990. Pp. xiv, 358. \$29.95.

Sheila L. Skemp has written a fascinating biography of the son of Benjamin Franklin. It is the story of a man caught between two irreconcilable forces. As royal governor of New Jersey in the thirteen years before independence, William Franklin had sworn fealty to the Crown, yet he had to answer the demands of a colonial assembly increasingly disenchanted with that Crown. He tried to please both forces and ended up satisfying neither. It is also the story of a once very close father-son relationship destroyed by their conflicting loyalties. Although William Franklin is the main subject of this biography, Benjamin is a close runner-up, for the filial relationship is the thread that holds the book together.

Skemp describes how William's character was formed by his relationship with Ben during his early manhood. As Ben grew in political prominence and economic independence, William became his friend and political cohort. When Ben was elected to the Assembly, William took his father's vacated position as clerk. When Ben became Postmaster General of America, William became Postmaster of Philadelphia. William went with his father to the Albany Congress, they invested in land together, and they shared a famous experiment with a kite. In 1757, father and son went to England, where William completed legal studies and secured the position as royal governor of New Jersey, an appointment that his father was immensely proud of at the time. Up to this point, William was confidant, companion, and assistant to Ben's projects, and both men were devoted, loyal subjects of the Crown. In 1762 and 1763, they returned to America, Ben to Philadelphia and William to New Jersey, but consultations were frequent as Ben advised his son on running New Jersey and William helped his father in the Pennsylvania Assembly's controversy with the Penns. Less than two years after William became governor, the Pennsylvania Assembly sent Ben back to England. Even during the separation, the ties between father and son continued to be strong. William took care of Ben's wife and daughter in Philadelphia, and Ben took care of William's illegitimate son in England.

But Ben arrived in London at the beginning of the Stamp Act controversy and began to shift his loyalties from England to the revolutionary movement in America. After receiving a public scolding from So-

licitor General Alexander Wedderburn for his role in the publication of some letters written by Thomas Hutchinson. Ben turned his back on England and became the spokesman and scribe of the revolutionary cause.

William, however, retained the loyalty to England that he had learned from his father and continued to believe that England, with all its faults, still offered the colonies more advantages than they would have as an independent country. William's inability to recognize any justice in the colonial cause cost him his job and the love of his father. He was arrested in June 1776, detained in Connecticut for two years, and sent into British-occupied New York City. By this time, father and son were estranged. William remained in New York until 1782, when he went to England. There he spent the rest of his life, never forgiven by Ben for his adherence to the British cause.

Skemp's narrative is controlled and graceful, and she has used extensive documentary sources as well as secondary works. The book is informative and engrossing.

ANNE M. OUSTERHOUT
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BOYD STANLEY SCHLENTHER. *Charles Thomson: A Patriot's Pursuit*. Newark: University of Delaware Press. 1990. Pp. 325. \$45.00.

Even popular revolutions do not just "happen." Between the speeches at one end and the commencement of armed conflict at the other, committees must be organized, constituencies cultivated, pressure groups formed. It was as a tactician, not as an original or even a consistent thinker, that Charles Thomson contributed to America's movement for independence.

Boyd Stanley Schlenther's narrative of Thomson's long and varied career is at its best when it describes his role as political organizer. Representative of America's rising middle class (aggressive, ambitious, determined to win status, honor, and economic security), Thomson valued means over ends, strategy over ideology. His goals were ultimately private, not public. If Thomson had any consistent philosophy, it was his national perspective, his "obsession with presenting to the world a unity of American purpose" (p. 205).

Thomson's apparent aimlessness led him to adopt and abandon often-conflicting positions. He attacked the Penn family's Indian policy in the 1750s but praised that same policy in the 1780s. He supported Benjamin Franklin's antiproprietary party until the Stamp Act crisis, when he deserted his former crony and allied himself with John Dickinson's antiroyal government coalition. Until 1774, Thomson was the leader of Philadelphia's "radical" mechanics. After he won his position as congressional secretary, Thomson switched gears, embracing a strategy of "tactical mod-

eration" (p. 133), actually retarding the movement for independence. And, although he supported the Federalist strategy for a stronger national government, he soon joined the ranks of Jefferson's Democratic-Republicans.

Thomson's record of achievement is mixed. This Ulster-born immigrant enjoyed his share of successes. His marriage to Hannah Harriston brought financial security and the status for which he hungered. His service as congressional secretary won him considerable prestige. Nevertheless, Thomson garnered only one elective office, a term in the Pennsylvania assembly in 1774. Although he served as secretary to the Congress until 1789, the new government refused to allow him to continue in that position, and he held no post—elective or appointive—up to his death in 1836.

Schlenther's research is careful and balanced, and his prose is clear, if uninspired. He makes a strong case for Thomson's significance. Thomson's story indicates that, in Philadelphia at least, there were those who longed for an American revolution, not just a war for independence. His ties with Philadelphia's revolutionary committees and his tedious work for the Confederation Congress throw some light on the mechanics of the revolutionary process. Moreover, Thomson was at the center of action from the 1750s until 1789. His secretarial post made him the only figure continually present in the various continental congresses from 1774 until the creation of the new government. Yet it is fair to ask whether a biography of Thomson adds significantly to the historical debate. Thomson destroyed most of his own papers, making a full assessment of the man impossible to achieve. More importantly, precisely because he was a tactician, not a thinker, his contributions to the revolutionary movement may have been significant, but they were neither unique nor decisive. His mere presence at so many pivotal junctures does not argue for his importance.

SHEILA L. SKEMP
University of Mississippi

ANTHONY MCGINNIS. *Counting Coup and Cutting Horses: Intertribal Warfare on the Northern Plains, 1738-1889*. Evergreen, Colo.: Cordillera. 1990. Pp. x, 258. \$14.95.

Almost everyone who writes on Plains Indian culture or history deals with intertribal warfare. Some assume, with La Vérendrye, that it had existed from "time immemorial," while others see it as largely a result of Euro-American pressures. Some represent it as almost a game, like medieval tournaments; others portray it in terms of massive campaigns aimed at the acquisition of territory.

Anthony McGinnis strikes a balance between these extremes. Intertribal warfare did exist in prehistoric times, he says, but it was intensified by white intrusion, especially in the nineteenth century. There was

an element of ritual about it, as reflected in the graded merit of different forms of individual contact with an enemy. But it also took on deadly serious manifestations, at least in prehistoric times, when strong tribes seized territory from weak tribes.

McGinnis's chronological approach begins with the first recorded European visits to the northern Plains and carries the story through major events such as the acquisition of the horse and of firearms by the Plains tribes; the devastating smallpox epidemics; the westward movement of the Sioux, with the consequent displacement of other tribes; and the intervention of the U.S. army, which ended intertribal warfare.

Accepting most of the prevailing interpretations, McGinnis does not break a great deal of new ground. In respect to two matters, however, he departs from orthodoxy. Contrary to older opinion, the addition of the horse and gun to the Plains tribes' cultural inventory did not lead to an increase in the scale of fighting, but instead, because of the likelihood of unacceptably greater casualties, reduced the incidence of major battles in favor of small-scale horse stealing raids. And he does not agree with many nineteenth-century observers that large tribes like the Sioux and Blackfoot were on the verge of exterminating smaller or weaker tribes. The question became moot, of course, with the suppression of intertribal warfare, but McGinnis believes that the emphasis on small raiding parties and light casualties would have insured the smaller tribes' survival.

The book's faults are minor, and most might have been avoided by more careful copy editing. On page 108 the author credits the Sioux with some 8,000 warriors in 1865, while on the next page he gives the "Sioux Alliance" only 5,000 fighting men, including Cheyennes and Arapahos, as of the same date. A difference of 3,000 men—more than the total fighting force of most of the rival tribes—is not trivial.

Mechanically the book does credit to Cordillera Press. The illustrations are well chosen and well reproduced, and the maps are adequate; neither are, however, listed in the table of contents. One of the book's most valuable features is a long bibliographical essay, which lists and evaluates numerous primary sources and secondary works. A graduate student or budding scholar interested in Plains Indian history would find this a useful place to begin.

If McGinnis does not add greatly to our knowledge of Plains Indian warfare, he does bring together in one place a great deal of information from widely scattered sources and offers reasonable, balanced judgments on disputed issues.

ROY W. MEYER
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CHRISTOPHER CLARK. *The Roots of Rural Capitalism: Western Massachusetts, 1780–1860*. Ithaca: Cornell University Press. 1990. Pp. xii, 339. \$32.50.

Christopher Clark has set out to demonstrate that social structures among farmers in the Connecticut Valley shaped the emergence of rural capitalism. Defining capitalism as "the set of social relations in which labor is commonly divorced from the ownership of the land, tools, or materials that form the means of production," Clark argues that "rural society was not, in this sense, 'capitalist' at the end of the colonial period but . . . it had become so by the time of the Civil War" (p. 14). Clark is not referring to the emergence of an agricultural wage labor force. Nor do factory workers or owners provide the focus of the book. Clark's subject rather is local farmers' acceptance of "capitalist social relations," or a capitalist *mentalité*. Farmers in 1780 were precapitalist because they were primarily motivated by family rather than profit; they used personal, face-to-face exchange mechanisms instead of cash; and they resisted participation in distant markets. Clark does not argue that most farmers were primarily motivated by profit in 1860, either (p. 309). The reader is thus left with the following proposition: the transition to rural capitalism occurred when farmers succumbed to the siren call of distant markets and acquiesced in the use of cash in the exchange of goods and services.

The roots of rural capitalism thus lay in two transitions: the shift from household production to a dependence on distant markets, and the increased use of cash in exchange. Although farmers participated in significant local trade in 1780, Clark argues that they were not really dependent on this trade. Because the trade was personal, it was under their control. Increasing pressure on the land, however, forced farmers to work harder to provide for their families. Much of this work load fell to women, who began purchasing textiles through the market. Most historians of women in America argue that store-bought textiles freed women from a great household burden and gave them new choices as to how to spend their time. Clark agrees, but he emphasizes that in doing so women had unwittingly taken the first step in dragging households into rural capitalism (pp. 142–43). Cloth could not be purchased without cash, and cash had to be acquired outside the home. From this point on households began to purchase more goods from distant markets until they were dependent on markets for their standard of living.

The second argument rests on the transition from local, personal trade relations to the use of cash in economic transactions. According to Clark, farmers consciously resisted using cash until the 1830s. Unfortunately, he uses an anachronistic concept of "cash" in developing this argument. There was no national paper currency before the Civil War, and hard coin was scarce. As a result, "cash" in antebellum America consisted mainly of bank notes, which could vary widely in value depending on the stability of the issuing bank. In particular, notes on distant banks could be highly risky. Clark does mention briefly that the number of bank notes increased nationwide after

1830 (p. 221). He never explains, however, how this would have played out in the Connecticut Valley. A number of banks were probably established locally after the 1820s. The increase in local banks should have led to an injection of local bank notes. Farmers simultaneously had access to more "cash," and to "cash" of a more stable value. It could be argued that this might have affected their willingness to use "cash" in trade. This is all a guess, however, because Clark provides no information as to the number of local banks or the type of bank notes used through the period. Even more curious, he omits any reference to bank notes in the early chapters pertaining to farmers' distaste for "cash" (see, for example, p. 67).

Finally, Clark's theses concerning the effects of cash and distant markets on local social relations and household independence cannot be separated from time and place. In colonial Pennsylvania, for example, a household-centered economy with traditional social relationships and a nonwage labor force easily coexisted with a cash market for produce. The region exported almost one-third of its farm produce overseas while importing a substantial amount of manufactured goods from England, yet farmers in the Delaware Valley displayed the same family-centered ethos as those in Massachusetts. Conversely, the personal, local, credit-dependent exchange relationships that characterized the rural South from 1870 to 1960 were hardly conducive to household independence.

Throughout the book Clark confuses simultaneity with causation. The sense of independence and community that Clark found in rural Massachusetts at the beginning of the nineteenth century was not caused by local markets, book exchange, and barter, although it may have coexisted with them. The cause, rather, is more likely to have been relative equality of wealth and income, relative equality of status and education, and a sense of shared history, culture, and purpose. And the introduction of cash and distant markets to local farmers did not cause industrialization in the Connecticut Valley. The changes in western Connecticut during the period of this study were brought about by cheap transportation in the form of improved roads, canals, and railroads; cheap labor in the form of immigrants; reduced opportunities for the farm population due to competition from Ohio and scarcity of land; and declining costs of capital as financial innovation allowed both local intermediation and an influx of eastern funds.

If the thesis surrounding rural capitalism is set aside, then the book contains an interesting and viable description of the reaction of farm families in western Massachusetts to the massive economic and social changes in the world around them between the revolution and the Civil War—changes that still appear to me to have been beyond their control. Although he may not have created a new paradigm for the study of early American industrialization, Clark has nevertheless made an important contribution to

our understanding of a particularly complex period of history.

MARY M. SCHWEITZER
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SIMON BAATZ. *Knowledge, Culture, and Science in the Metropolis: The New York Academy of Sciences, 1817–1970*. (Annals of the New York Academy of Sciences, number 584.) New York: New York Academy of Sciences. 1990. Pp. ix, 269. \$55.00.

This sponsored history of the New York Academy of Sciences is in fact much broader than most institutional studies. Simon Baatz uses the academy—through most of its 150 years a small, multifunctional organization—as the focal point for a wide-ranging and pioneering account of the institutions and personalities that shaped science in New York City. Yet the book does not quite realize the promise, expressed in its title, to characterize the particular nature of science in the American metropolis.

The New York Academy of Sciences began in 1817 as the Lyceum of Natural History, a local center for polite scientific learning. It adopted its present name in 1873, a few years after a fire destroyed its collections of specimens, and the American Museum of Natural History preempted renewed activity in that direction. The academy functioned primarily as a meeting ground for individuals associated with New York's universities, museums, and specialized scientific clubs. It transcended that local role in the 1940s by using New York City as the site for a series of international conferences designed to publicize pharmaceutical and other technoscientific advances. At present it combines the organization and publication of conferences with high-level popularization, most notably through its magazine, *The Sciences*.

Prior to the 1940s the academy was a source of contacts and status within the local scientific community, but a chronic supplicant to its stronger neighbors for funds, space, and often for something to do. The academy's history therefore comprises a meandering path from which Baatz is able to survey the development and interactions of many institutions. These include Columbia University, New York University, City College, the College of Physicians and Surgeons, Cooper Union, the American Museum of Natural History, the New York Botanic Garden, the New York Public Library, and other less well known entities. The book also describes the activities of New York's scientific leaders, from Samuel Latham Mitchill and John William Draper to Henry Fairfield Osborn and Franz Boas.

Like many historians of American science, Baatz focuses on organizational politics as the bridge connecting the seemingly insignificant naturalists of the nineteenth century with the world-shattering enterprise of the last fifty years. This emphasis does not, however, provide a strong basis for addressing the

more pertinent issue of the texture of scientific activity within the American metropolitan context. An episode in the history of the academy that Baatz describes without much comment illustrates some interpretive possibilities. In 1912, the academy began a twenty-year comprehensive natural history survey of Puerto Rico, funded through a mix of private New York and public Puerto Rican funds. This expression of nostalgia for the era of John Wesley Powell provided professors a way to work outside the city during summers and semester breaks; it benefited Puerto Rico and at the same time fostered the special form of cultural imperialism that bound the island to New York. Three metropolitan values—leisure, benevolence, and power—were thus linked in the particular kind of science fostered by the academy. This triad seems a workable base from which to reconstruct much of the development of science in New York.

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RONALD E. SHAW. *Canals for a Nation: The Canal Era in the United States, 1790–1860*. Lexington: University Press of Kentucky. 1990. Pp. x, 284. \$28.00.

The Argentine educator, Domingo Sarmiento, once observed that if God called the world to judgment in 1847, “he would surprise two-thirds of the population of the United States on the road like ants” (Michael Rockland, trans., *English Travels in the United States in 1847* [1970], 133). Ronald E. Shaw explains why many of them would have been gliding across the American waterways on the upper decks of canal boats.

Shaw outlines two routes in his book. He explains the history behind the planning and construction of the major canal arteries, and he puts the canal movement in the context of its times. His recitation of regional canal construction is pedestrian, whereas his discussion of canal promoters, men such as George Washington, Henry Knox, Philip Schuyler, Robert Morris, Thomas Jefferson, James Madison, James Monroe, and John Marshall, is fascinating. Canal building, Shaw asserts, appealed to Americans who thought in terms of a “water-connected society” and who supported government encouragement of economic growth.

Even the Jeffersonians advocated public financing of canals to unify the nation and to combine the public with opportunities for private gain. The canals were therefore mixed enterprises. A few were built with private capital, but most were constructed with a combination of private and public funds or entirely from public treasuries.

Canal promoters often built bipartisan consensus in the states that enabled them to push appropriation bills through legislatures. Some states, however, overreached their financial resources; in Indiana, the

strain of building the Wabash & Erie Canal in the depression was so severe that the state had to default on its bonds. Illinois followed suit, and Ohio, with ambitious improvements that included the Miami & Erie and the Ohio & Erie canals, escaped default by a hair’s-breadth.

Most canals failed to show a profit, but Shaw argues that their social returns far outweighed their costs. They provided jobs for the newly arrived unskilled Irish and German immigrants, created a long-distance transportation network, knit sections together, fertilized cities such as Chicago, Rochester, Akron, and Fort Wayne, and carried along on their almost-still waters ideas, reforms, and diseases. Moreover, Shaw contends that the canals began the economic realignment that diverted western traffic from New Orleans to the East Coast and tied the West politically to the East.

Shaw views the canals as more than a mere interlude between the turnpike era and the railway age. He believes that canals and railroads were symbiotic developments. Methods for raising canal capital transferred easily into the railroad industry, and engineers trained on the Erie and other projects found their way into the railway world. The political coalitions, the canal surveying technology, and the belief that all new transport was in the public good enabled iron rails to conquer the nation’s vast spaces. The canals, however, did not immediately bow to steam. Not until 1854, Shaw asserts, did railroads move more freight tonnage than canals. And, on the more prosperous Erie, tonnage did not peak until 1874, long after it was paralleled by railways. This survey, with its excellent maps and engaging writing style, is an enjoyable and informative read.

JAMES A. WARD
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TIMOTHY R. MAHONEY. *River Towns in the Great West: The Structure of Provincial Urbanization in the American Midwest, 1820–1870*. New York: Cambridge University Press. 1990. Pp. xi, 319. \$39.50.

One of the dominant paradigms for nineteenth-century U.S. history has been the rise of the market economy, especially the social context and social consequences of that great transformation. In this book, Timothy R. Mahoney also focuses on the dynamics of market expansion but concentrates on the spatial rather than the social dimensions of that process. Informed by the methods of historical geography, especially the work of Allan Pred and Michael Conzen, this study is a detailed social-scientific analysis of the transformation of the upper Mississippi River Valley between 1820 and 1870 from an unsettled frontier to a regional urban system.

Mahoney attempts to transcend some of the limitations of the more traditional mode of urban history,

that is, the individual city biography, and illustrates how many local developments were actually determined by larger changes and forces in the region as a whole. Although the analysis is interdisciplinary and methodologically sophisticated, there is not enough sense of the relevant larger issues, with respect either to historiography or to social-scientific theory. In particular, the author furnishes no preface or introduction to give the reader guidance about where this book is going and why.

The study is divided into three sections that proceed chronologically. The first deals with the settlement process and discusses the relative importance of different factors such as soil quality and access to markets in determining where settlers moved. The physical characteristics and environmental rhythms of the rivers themselves, such as shifts from high water to low, also influenced settlement patterns and continued to affect the contours of an emerging regional system.

In the second section, Mahoney discusses the progress of this system after settlement and focuses on the interplays among market forces, the development of transportation networks on land as well as water, and the growth of different communities. By mid-century, a regional economy based on lead, wheat, and corn and hog production had materialized, and Mahoney describes the interactions between fluctuations in each of these markets and the different features of the region's cities.

The last section of the book, entitled "The Regional Urban System," looks at the changing fortunes and characteristics of the various cities in the region as they adapted or failed to adapt to larger forces of change. Here Mahoney makes a compelling case for the need to understand local history within a regional context and offers some intriguing speculations about the possible connections between his geographic and economic study and the social history of the region—implications that, alas, he does not develop in this book.

This work is an innovative study that should be of interest to both students of midwestern history and specialists in historical geography. Its lack of a fully integrated conceptual framework, however, is a limitation. It is ironic, perhaps, that a work dedicated to providing a larger regional context within which to better understand local developments fails to provide its readers with a larger theoretical or historiographical context within which to place its findings.

HAL S. BARRON
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FRANCES B. COGAN. *All-American Girl: The Ideal of Real Womanhood in Mid-Nineteenth-Century America*. Athens: University of Georgia Press. 1989. Pp. x, 298. Cloth \$35.00, paper \$14.95.

"The cult of true womanhood" has so long served as the central metaphor of nineteenth-century U.S. women's history that all challenges to its hegemony are to be warmly welcomed. This book by Frances B. Cogan is such a challenge. It is not an outright assault; like the historians with whom she takes issue, Cogan is more interested in prescription than in behavior and her focus remains steadily on the middle classes. It is all the more intriguing, then, that she rejects "true womanhood" as the reigning American Victorian model and identifies in its place an ideal not of "true" but of "real womanhood." To be sure, the two paradigms shared some ground—most important, both represented women chiefly in terms of familial roles: good daughters, good sisters, good mothers, good wives. And yet they offered young women of the middle classes quite different road maps for maneuvering toward and through adulthood. Where "true womanhood" celebrated passivity and obedience, "real womanhood" praised women who demonstrated resourcefulness, autonomy, and pragmatism—women who eschewed delicacy for sturdy good health, who developed their mental as well as their maternal capabilities, who picked their ways unsentimentally through the minefields of courtship, and who prepared themselves with saleable skills against the possibility that no appropriate suitor would appear.

Tracing "true womanhood" to European roots, Cogan argues that "real womanhood" was the authentic American ideal. Not only did the emerging middle class find anything that smacked of "leisure" repugnant to its norms of hard work and frugality but also the roller-coaster economy of the mid-nineteenth century put anything that resembled actual leisure well beyond the reach of most Americans. "Real womanhood," then, was a response both to fears of "old world" corruption in the cultural life of the new republic and to the practical problems of growing up female in the United States. As such, Cogan insists, it expressed an internally coherent and fully independent prescriptive type, an American ideal that was praised not only in the treatises of fringe movements but throughout the pages of mainstream advice books and fiction as well.

Cogan gives as one reason for writing this book that her own reading of nineteenth-century popular literature seemed at odds with the prevailing historiographical emphasis on the "cult of true womanhood"—an experience shared by other women's historians, I suspect. Facing down adversity with aplomb, making their own decisions and charting their own lives, many of the heroines of antebellum fiction seem far removed from the submissive creatures identified by Barbara Welter as the ideal women of mid-nineteenth-century America. One might want to argue over whether "real womanhood" was a fully autonomous oppositional ideal (ideals are always modified in behavior and "real womanhood" may simply have represented the adaptations of "true

womanhood" to daily experience). Still, Cogan begins the process of distilling the heavenly essences of "true womanhood" to the materiality of middle-class life in the nineteenth century and for that one is extremely grateful.

JEANNE BOYDSTON
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CAROL A. KOLMERTEN. *Women in Utopia: The Ideology of Gender in the American Owenite Communities*. Bloomington: Indiana University Press. 1990. Pp. x, 209. \$29.95.

Carol A. Kolmerten has written an important and, in some ways, a troubling book. Recognizing that the contemporary debate about gender roles must be set in historical context, she examines the gap between nineteenth-century Owenite professions about gender equality and the reality of women's lives in communities such as New Harmony, Indiana; Blue Spring, Indiana; and Equality, Wisconsin. She concludes that even utopian socialists who claimed that the liberation of women was a goal rarely, if ever, achieved it.

Kolmerten seems surprised to find that, in practice, patriarchal attitudes triumphed over the abstraction of gender equality. This lends something of an anachronistic tone to her argument. In describing the experience of John Hudson, a member of the Promisewell community, for example, Kolmerten notes that he "gushed" about the group's female members and "couldn't help labeling [them] 'virtuous'" (p. 153). She considers Thomas Steel's wish to have women attend to his laundry and mending inappropriate, arguing that he wished to be "coddled" (p. 156). Why, Kolmerten seems to ask, could Hudson and Steel not free themselves from the mainstream conception of "true" women?

Their female fellow communarians experienced similar difficulties. Even after women were granted the right to vote at New Harmony, few took advantage of the opportunity (p. 83). When leaders there decided to place older children in boarding school, several women rebelled at the breakup of the traditional families in which they had exercised considerable, although unofficial, authority (p. 96). Few Owenites wished to do away with patriarchy entirely. After all, class, not gender equality, was at the center of nineteenth-century utopian socialism. To suggest that women's rights should have been to impose a contemporary value system on the past.

Nevertheless, Kolmerten's story is important. The rhetoric of equality espoused by nineteenth-century utopians is seductive. "Our Object," stated the New Harmonites in their constitution, "is happiness. Our principles are Equality of Rights, uninfluenced by sex or condition" (p. 37). The appeal of such a statement is obvious. But what was the reality of life in Owenite

communities? It is here that Kolmerten has made her most valuable contribution.

Kolmerten describes the efforts of women such as Frances Wright, Marie Fretageot, Sarah Pears, Eliza M'Knight, and Camilla Wright to live fulfilling lives in communities whose ideology and social structure continued to circumscribe their "sphere." The stories are, for the most part, depressing. Even Frances Wright and Marie Fretageot, whose lives in community produced some successes in the areas of female leadership and education, were marginalized to a large extent. These women generally found life in community unsatisfying because, while they were "allowed" to work full time in community industries, they were expected to perform most, if not all, of the domestic work as well (p. 79). This was the promised "equality."

For the Owenites, as for Americans today, it is difficult to mix equality and domesticity. American society and culture are still imbued with patriarchal attitudes and values, which, as Kolmerten observes, makes it "hard to take women seriously." She hopes that a truly egalitarian world, such as that "envisioned if not enacted by Owen and his followers" will, someday, transform women's lives (p. 176).

PRISCILLA J. BREWER
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JANE H. PEASE and WILLIAM H. PEASE. *Ladies, Women, and Wenches: Choice and Constraint in Antebellum Charleston and Boston*. (Gender and American Culture.) Chapel Hill: University of North Carolina Press. 1990. Pp. xiii, 218. Cloth \$24.95, paper \$10.95.

Jane H. Pease and William H. Pease take historians of women another step along the path away from the constrictive ideology of separate spheres as a method of interpreting women's lives. They choose instead a spectrum—on one end, full autonomy; on the other, complete dependence—and they argue persuasively that most women's lives in the important cities of Charleston and Boston during antebellum America ranged along the spectrum instead of being confined to either end. Within the limits of class, race, and social prescription, women did have the ability to choose. And the authors present a solid portrait of both the choices available and the constraints under which women made their choice.

To marry or not was the primary choice for women, because for the vast majority marriage defined their lives and limited their options. Marriage, however, was part of a circle from spinsterhood to widowhood: from a degree of autonomy to a degree of dependence to a degree of autonomy. Within this circle fell the rest of the choices available to women—the economic choice of work at home or wage-earning labor either at home or in the public sphere; the choice of reform activities in the church, in voluntary associations, or in public organizations; the choice of education limited to domestic concerns

or incorporating a wider world. All of these choices, however, were made within the constraints of public expectations regarding female behavior. Some women bowed to constraints and enjoyed respectable lives; some resisted and suffered the consequences; many, the authors argue, resisted some restraints when they wanted to or needed to and went along with other restraints in order to live fulfilling lives. The style of women's reform efforts in Boston, for instance, stemmed from the private nature of welfare institutions in that city. Because charity was not a task of the government, women could participate in public forms of charity more easily and could organize independent female assistance societies as well. The governmental link with charity in Charleston, however, limited women's actions there to fund raising and reform efforts under the protective and restrictive umbrella of male-dominated organizations. But in both cases, reform movements were not merely a way women chose to resist constraints on their activities. They could also be, for some women, a way to promote the preservation of the family and social conventions against the vicissitudes of life.

The authors' argument is solid and their structure is sound. Their sources include a general census taken in Boston in 1845 and a census from Charleston in 1848. They try, with some success, to include all classes and both races in their analysis. But their work is simply too short. Based no doubt in part on their research for their earlier book, *The Web of Progress: Private Values and Public Styles in Boston and Charleston, 1828–1843* (1985), this new work shows a lack of depth that is occasionally disconcerting. Only twelve pages on prostitution—the “Wenches” of the title? And only ten pages on sexuality? Surely both topics deserve fuller treatment, particularly given the link between women's sexuality and social perceptions of what made women “wenches.” The bibliographic essay provides a solid, if uninspired, reading list of appropriate and important works. But calling Barbara Welter's famous article on the cult of true womanhood “seminal” does make a reader question the authors' overall sense of the field of women's history.

The delineation of the differences between women's lives in antebellum Boston and Charleston does help make up some of the lack of depth in this work, and the comparative framework and spectral analysis provided are useful for future historians to work with. The approach is sound, the execution wanting, but *Ladies, Women, and Wenches* does give a glimpse of what can be done.

JANET L. CORYELL
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JOY S. KASSON. *Marble Queens and Captives: Women in Nineteenth-Century American Sculpture*. New Haven: Yale University Press. 1990. Pp. xviii, 293. \$40.00.

Joy S. Kasson's book is a welcome addition to studies in the history of nineteenth-century American art. It is particularly valuable because the author examines sculpture, which has attracted few scholars and is still generally understood through documentary rather than interpretive studies. Kasson remedies this lack, giving readers a rich, subtle, and provocative argument about the relation of certain marble sculptures from 1840 to 1880 to cultural preoccupations.

She begins with two chapters on the sociology of sculpture and of viewing at mid-century, examining specific artists and their patrons and looking closely at the physical circumstances in which American viewers experienced sculpture. With this context established, she then looks specifically at the production in significant numbers of marble figures of classical and Biblical queens and of female captives—figures based on history, mythology, literature, and allegory. The popular subjects include Zenobia, Eve, Pandora, Medea, and Hiram Powers's famous statue *Greek Slave*, all themes that present women as vulnerable, licentious, foolish, passionate but without judgment, and/or submissive. Done by American sculptors, most of whom worked in Italy, including Powers, Harriet Hosmer, Thomas Crawford, and William Wetmore Story, the works were commissioned by or sold to prominent American patrons such as the New York department store magnate A. T. Stewart and Boston merchant Thomas H. Perkins. Reviewers wrote generously of these sculptures whether reporting exhibitions or purchases.

Each theme invoked by these subjects, Kasson argues, makes a statement understood in its time to be about woman's nature. Thus, each sculpture embodied a position—often a deeply conflicted one—in the cultural debate about the domesticity, sexuality, and political and social freedom of women. Kasson's entry into her exposition is through the narratives that critics and other viewers spun about the sculptures. Rather than limit her probe to iconographic precedents, formal language, and technique, as art historians typically have done, she uses the language of reception—from newspapers and journal reviews, catalogue descriptions, and correspondence—and brings to its interpretation her skills as a literary scholar. Kasson shows that within the narratives with which men and women interpreted the visual language of the works, the most powerful cultural assumptions, ideologies, and conflicts may be found. Although verbal evidence dominates Kasson's study, however, she pays attention to visual clues as well, linking them to the interests in class and gender of the producing sculptor. Looking at the works themselves, for instance, she points up the different definitions of woman's “nature” by male and female sculptors. Her most powerful examples include Hosmer's depiction of the captured Syrian queen Zenobia as introspective and even autonomous (and thus sexually unyielding to her Roman captors) and Powers's celebrated presentation of a young woman slave

as simultaneously sensuous, chaste, and—because she is chained—available.

The drawback of Kasson's study is one she would freely admit. She declares forthrightly in her introduction that she examines selected objects rather than the broad range of sculptural production. This means that she does not summarize the output of each sculptor as well as the general run of works on exhibition at any one time. What, the reader is hungry to know, are the proportions of the works she examines to the larger context of sculpture? What relation is there between the characteristics attributed to women in ideal sculpture and those attributed to men? What are the narrative strategies that critics employed in interpreting sculptural themes other than those of woman's nature? These reservations notwithstanding, Kasson makes an important contribution to the study of American sculpture, American cultural history, and the history of the construction of gender.

ELIZABETH JOHNS
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HARLAN HAGUE and DAVID J. LANGUM. *Thomas O. Larkin: A Life of Patriotism and Profit in Old California*. Norman: University of Oklahoma Press. 1990. Pp. xiii, 304. \$28.95

This is a fascinating book about Thomas O. Larkin, a fascinating man. Long lacking a satisfactory biography, this businessman and statesman who seemed to play a major role in the American acquisition of California has intrigued scholars, although it has never been completely clear just what that role was. The voluminous *Larkin Papers* (1951–68) edited by George P. Hammond have thrown much light on the activities of Larkin, but we have always needed a thorough study of the man behind the papers. Harlan Hague and David J. Langum, both seasoned scholars, have now slaked our thirst by giving us a full-length biography of Larkin based on the *Papers* and many other sources, some newly discovered.

The portrait that emerges shows us a nineteenth-century man-on-the-make, in more ways than one, who seized his opportunities for himself and for his country. Sailing to California from his native Massachusetts in 1832 to work with his half-brother in the lucrative hide and tallow trade, he seduced and impregnated another man's wife in the course of their journey or shortly after their landing. When the woman's husband conveniently died *in absentia*, Larkin married her but only after determining that the young widow had received a substantial inheritance from her dead husband's estate. This windfall provided the foundation of Larkin's eventual huge personal fortune, a product not only of luck but of ceaseless industry and canny business sense on his part.

The authors provide ample evidence, however, that Larkin was no mere cad and money grubber. His

marriage was a love match, and he had lasting and affectionate relations with his children. Unlike many others of his class, Larkin did not merge into the *Californio* population by intermarriage, taking Mexican citizenship, converting to Catholicism, and becoming a *ranchero*, but he did enjoy excellent relationships with the Spanish-speaking populace (he spoke and wrote Spanish fluently) and was genuinely sympathetic toward and appreciative of the Latin life style. An unabashed American patriot, Larkin accepted an American consulship, advocated California's joining the United States, and accepted an appointment as President James Polk's confidential agent. These actions did not alienate him from his California associates. After all, his undeviating strategy was to induce the Californians to voluntarily join the United States, and he was appalled and embarrassed by John C. Frémont's blundering and the aggressions of the Bear Flaggers.

When the deed was done, however, Hague and Langum clearly show how Larkin not only accepted California's new status as an American province but also profited enormously from the change by plunging back into a business career, especially in land speculation. We are much indebted to the authors for their explication of how to make one's interests correspond with one's principles and win fame and fortune in the process.

JACKSON K. PUTNAM
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ALFRED RUNTE. *Yosemite: The Embattled Wilderness*. Lincoln: University of Nebraska Press. 1990. Pp. xii, 271. \$24.95.

In 1864, the U.S. Congress voted to preserve from settlement two scenic landscapes, the Yosemite Valley and Mariposa Grove of giant sequoias. The state of California took responsibility for safeguarding these treasures, but then, in 1890, they reverted to federal ownership and became a national park, nearly one million acres in size. Since then the National Park System has grown to include nearly 400 units today. Beginning without any precedent, Americans had no clear idea as to how the parks ought to be used. In the view of Frederick Law Olmsted, the codesigner of New York City's Central Park and one of the leading figures in the initial preservation of Yosemite, the goal should have been strict protection of the natural landscape. Others, however, wanted to sell the scenery to as many people as possible, and quickly. Even before Congress acted, there were ambitious settlers coming into the valley to get possession and go into the tourist business.

This book by Alfred Runte is the best study we have of the conflict over management objectives in the national parks since the 1860s. Before the coming of white settlers, the local Native Americans burned the

valley floor regularly and created an open landscape that stunned the senses of Olmsted, John Muir, and a succession of painters, photographers, and other travelers who found their way into this remote backcountry. But the whites suppressed the fires, and in a few decades a thick growth of trees blocked their view of the imposing granite walls. They introduced exotic fish and game species, and even set up a zoo. And they slaughtered the dangerous, "killer" animals, including grizzly and black bears, mountain lions, wolves, coyotes, and rattlesnakes. Tourists could not gape and admire if they were running for their lives. If it was permissible to eliminate such dangers, then it was also permissible to enhance amenities. Entrepreneurs such as David Curry and Jeanne Curry catered to the wants of tourists, providing a campground, then a hotel, then another, a bowling alley, movie theater, golf course, dance hall, barroom, and so forth. Evidently the public liked these changes in the landscape, for by the late 1980s there were 3.2 million visitors each year. What they were getting was more commercial resort than nature. Fittingly, in 1973 the Curry Company, owner of the park's concessions, became part of the Music Corporation of America's entertainment empire. In that same year reporters discovered the dump where park officials disposed of the bears they had shot—more than 200 of them in a single decade. Runte argues that Yosemite, like many other parks, has served the demands of profit, self-interest, and mass tourism far more than the ideals of wilderness preservation and sanctuary.

All this is not unknown. Runte makes a significant contribution, however, in tracing the development of an alternative management ideal aimed at preserving the park's biological heritage and educating the public in the ways of nature. The key figure here, whom Runte calls "the conscience of the park," is, surprisingly, neither Olmsted nor Muir but Joseph Grinnell, director of the Museum of Vertebrate Zoology at the University of California, Berkeley. From 1914 to 1939 he tried to direct park officials away from their obsession with visitor count and toward an ethic of protecting the indigenous plants and animals in a state approximating what the place had been before white "discovery"—creating a "university of the wilderness." Whether this ideal was in fact a condition of wilderness or another human-dominated landscape (the environment of the Native Americans) is not a question that Runte explores very deeply. But his main argument is persuasive: Americans have only very slowly and partially heeded Grinnell's advice. They may have invented a wonderful thing in the national parks, but they have sacrificed much of the parks' intrinsic beauty to fun and money.

DONALD WORSTER
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VICTOR B. HOWARD. *Conscience and Slavery: The Evangelistic Calvinist Domestic Missions, 1837–1861*. Kent,

Ohio: Kent State University Press. 1990. Pp. xv, 263. \$27.50.

Victor B. Howard asserts that northern evangelical Calvinists played a major role in mobilizing popular antislavery sentiment. He thus demolishes the claim, occasionally heard, that Calvinists were characteristically soft on slavery. Howard concentrates on the influential New School Presbyterians and Congregationalists who cooperated in evangelizing the nation and attempting to shape its ethos, especially through the American Home Missionary Society and its rivals and offshoots. Howard has conducted prodigious research in the records of these voluntary societies, in regional church records, and in religious periodicals.

Whereas some northern Calvinists, notably Old School Presbyterians, were indeed tolerant of slavery, evangelical Calvinists were largely divided between moderates and radicals. Such conflicts were played out in the American Home Missionary Society, which resisted a strong antislavery stand until the mid-1850s, and in related agencies and denominations. From Howard's account, it becomes clear that decades of abolitionist agitation eventually resulted in solid antislavery stances by these groups during the 1850s. Congregationalists, who had to deal with no southern constituency, unified on the point earlier than did the New School Presbyterians; but by the time the Republican party was founded in 1856, northern evangelical Calvinist organizations and publications were strongly arrayed in insistence that there should be no moral compromise.

Howard relates this gradual mobilization of religious opinion to the political events of the 1840s and 1850s. Particularly important is his account of how evangelical Calvinist antislavery agitation, especially in the Midwest, laid the groundwork for the organization of the Republican party. The network of antislavery clergy and laypeople was active in organizing the new party and preaching its cause, especially in 1856. Howard also argues that evangelical Calvinist leadership had little sympathy for the nativism of the Know-Nothings.

Although Howard makes a convincing case for these points, they are not quite as novel as he suggests. The themes are familiar, for instance, in studies of the Beechers or of antislavery leaders such as Jonathan Blanchard. Howard fills in a mass of detail that places such well-known figures in a larger picture of a successful campaign to arouse religiously based moral indignation. Unfortunately, because Howard limits his study to one religious type, it is difficult to judge their relative influence. There is also little way to measure the impact of religious versus more secular influences in shaping popular opinion. Howard's research, however, adds weight to any case for religious influences. Like so many books in the field, this volume's strength in research is not matched by its presentation. The method is largely that of multiplying instances.

The point of view is also puzzling. While his entire presentation centers around proving how many evangelical Calvinists were calling for less compromising antislavery stances, Howard finally laments that a moderate solution was not found and concludes cryptically that "The lesson for contemporary society seems to be that . . . questions of a moral nature on which society does not agree would best remain, if possible, outside the realm of politics" (p. 189). This seems an odd concluding thought for a book about slavery.

GEORGE M. MARSDEN
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JAMES W. OBERLY. *Sixty Million Acres: American Veterans and the Public Lands before the Civil War*. Kent, Ohio: Kent State University Press. 1990. Pp. xii, 222. \$28.00.

James W. Oberly has produced an important monograph effectively showing that the bounty land-grant system of the 1850s for veterans was more of a success in terms of fairness and efficiency than historians have previously believed. Earlier studies concluded that mid-nineteenth-century land policy was not only wasteful but harmful to the interests of settlers. Oberly shows instead that while the system did not always produce the results confidently predicted, it did accomplish what Congress sought to a remarkable degree.

With the Ten Regiments Act of 1847 and three laws in the 1850s, Congress abandoned the old distribution system of selling public lands and replaced it with a system of free land, first for veterans and, finally, with the Homestead Act of 1862, for all Americans. The veterans' laws provided grants to more than 550,000 people amounting to sixty million acres, the approximate size of Wyoming. This result was in large part because of the pressure of a veterans' lobby, the United Brethren of the War of 1812, whose members stood to gain the most.

Only a tiny percentage of the veterans affected actually settled on bounty lands; most instead sold their warrants. But this fact did not challenge the effectiveness of the program. Oberly explains that the stated aims of Congress were that the public domain should be turned into private property and that land policy should aid the settlement of farmers, and the legislation accomplished these goals. Although most sold their warrants to speculators who in turn sold them to interested settlers, the profits the land jobbers reaped were not excessive. Moreover, the cash that the veterans received was significant and the prices paid by actual settlers were reasonable. The study traces the efforts of the Pension Bureau, which determined eligibility of those applying for warrants, and the General Land Office, which located the grants. Oberly looks at the whole process from the creation to the issuing and disposition of the land

warrants. His results are based on a thorough survey of primary sources, including the records of the federal offices involved, samples of warrants, and information found in both federal and state archives. Only the specialist will be interested in the wealth of statistical data on how veterans acquired and used their warrants, but all who are concerned with western settlement and antebellum politics will find the study important.

In places Oberly goes further in his conclusions than the evidence permits. He is correct in suggesting that sectional politics were not critical to the land bounty issue, but he de-emphasizes the division between North and South too much. To suggest that James Buchanan vetoed a homestead bill in 1859 because many veterans had not yet had time to profit from the sale of their warrants while ignoring the president's belief that free land would benefit northern farmers more than southern slaveholders is a questionable interpretation at best. Originally appearing in the Free Soil platforms of 1848 and 1852, the issue was more sectional than the author admits. Yet, for the bills that preceded the Homestead Act of 1862, Oberly makes an effective case in showing their nonsectional character and their overall effectiveness in permanently changing the direction of American land policy.

FREDERICK J. BLUE
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KENNETH M. STAMPP. *America in 1857: A Nation on the Brink*. New York: Oxford University Press. 1990. Pp. ix, 388. \$29.95.

In 1857 the Supreme Court and President James Buchanan intended to settle once and for all the legal status of slavery in the western territories. The story of the controversial Dred Scott case and the LeCompton constitution imbroglio is well known. And what is argued here squares with the up-to-date historical literature. So why has Kenneth M. Stampp, a renowned historian of the Civil War period, written a book about 1857?

In this magnificent portrait of late antebellum American society, Stampp establishes beyond a reasonable doubt the unequivocal moral and political bankruptcy of the Buchanan administration. The background to the main drama is fleshed out in vivid detail—the financial panic of 1857, urban street riots, congressional corruption, the filibustering campaign against Nicaragua, and Mormons preparing to fight federal troops in Utah. All these events proved to be spasms on the surface while the conflict over slavery in the Kansas Territory shook national politics from beneath. Although Chief Justice Roger B. Taney's appallingly partisan Dred Scott decision inflamed northern antislavery forces, it left in theoretical limbo the question of slavery's actual gains. More politically damaging was Buchanan's endorsement of the entire

Lecompton swindle, for it threatened to result in a concrete advantage for the South. The president's inept handling of the flare-up over Kansas fatally split his own party and unwittingly played into the hands of the Republicans, "who could not have been better served if they had themselves written the script" (p. 14). The political crisis of 1857, according to Stamp, proved to be the decisive factor in preventing a peaceful resolution to sectional strife.

Stamp's book invites two questions. First, why did Buchanan renege on his commitment to a referendum on the entire constitution and make Lecompton a test of party loyalty? The president understood that southern Democrats were not unvaryingly opposed to fair elections in Kansas. He also knew that most northern Democrats considered the actions of the proslavery minority a defilement of the popular sovereignty doctrine. Buchanan was not held hostage to legalistic doctrines, browbeaten by his southern advisers, or frightened by "fire-eating" rhetoric. Attributing all evil in the territory to the antislavery forces and believing his course of action to be judicious and patriotic, he incredibly never regretted his decision to trample on the popular will in Kansas. Second, if Buchanan had demanded that the entire Lecompton document be submitted to the Kansas voters, then would secession and probable civil war have been averted? Stamp correctly states that "no historian can answer" this counterfactual question (p. 330). A danger exists here in that general readers might conclude that, but for Buchanan's blunder over the Lecompton constitution and his tragic failure of leadership, the Civil War could have been avoided. Stamp is not discounting John Brown's raid on Harper's Ferry in 1859 or the breakup of the national Democratic party in 1860. His contention is that after 1857 the North and South had probably reached "the political point of no return" (p. viii). In looking back, 1857 is for the author the year after which "discussion and negotiation gave way to accusations and inflexible demands" (p. 322).

This is grand narrative history, with all quantitative wizardry, social-science jargon, and ethnocultural theories concerning the cause of the Civil War mercifully leached out. Stamp offers mesmerizing glimpses into behind-the-scenes politics and everyday life, but he has much more to tell than feebly linked anecdotes. Attention invariably returns to the conflict over slavery, which constituted nothing less than the very "heart of the matter" (p. 110). Always in full control of his sources, mainly newspapers and private manuscripts, Stamp has added to his list of distinguished scholarly works the clearest and most comprehensive vision yet produced of the United States in 1857, the year this nation took a giant step toward disaster.

DALE BAUM
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IVER BERNSTEIN. *The New York City Draft Riots: Their Significance for American Society and Politics in the Age of the Civil War*. New York: Oxford University Press. 1990. Pp. ix, 363. \$29.95.

During five days in July 1863, violent and riotous crowds of working-class New Yorkers shut down shops and factories, battled police and soldiers, destroyed industrial and business property, torched the homes of the wealthy, and indiscriminately beat and killed blacks. What began as a protest against Civil War conscription quickly turned into a vicious race riot, an attack on local institutions, and a major challenge to the wartime authority of Abraham Lincoln's Republican party. The draft riots have been written about before, most notably in Adrian Cook's *The Armies of the Streets* (1974), a narrow case study of the event. Iver Bernstein's book, however, bears little resemblance to that earlier work. In fact, this new study ranges far beyond the riot of 1863—backward into the 1850s and forward into the 1870s—in order to "situate the draft riots in the context of an ongoing process of urban change" (p. 6). In so doing, Bernstein has written not just a book about the New York City riots but a major analysis of the political and social structure of the mid-century metropolis in the midst of dynamic change as well.

Bernstein rejects the generally accepted interpretation of the New York City draft riots as simply an episode of Irish Catholic hostility toward blacks or white working-class anger over the unfairness of conscription legislation. These elements of conflict were present, to be sure, but much more was happening in New York that helps explain the violent crowd behavior of July 1863. For Bernstein, three important issues dominated political and social thinking in mid-nineteenth-century New York—conflicts between rich and poor, between whites and blacks, and between the Democratic-controlled city government and the Republican-controlled federal government. These issues of class, race, and power provide the conceptual framework with which Bernstein explores the changing social, economic, and political pattern of New York City. They also lead to the author's analysis of the draft riots as class conflict, racial explosion, and "communal upheaval against the power of an expanding and centralized federal government" (p. 4).

The book is organized into three sections. The first provides a narrative of the week-long draft riots, focusing on working-class grievances and diverse responses from business and political elites, rising industrialists, and small business proprietors and contractors. The second section sees the origin of the crisis in the politicized struggles of urban workers, conservative merchant and business leaders, and radical Republican reformers during the 1850s. The final section traces the rise and decline of Boss Tweed's Tammany Hall after 1865, offering a stimulating new analysis of New York machine politics as a political resolution of the multiple conflicts that

surged to the surface during the war years. These last two sections, buttressed with wide-ranging research and substantive detail, are highly innovative.

This book demonstrates the complexity of mid-nineteenth-century urban life at every level. Bernstein digs into the working class, exploring the changing urban economy through the eyes of artisans, industrial workers, and common laborers. These groups had different perspectives, stemming from varied ethnic cultures, craft practices, shop-floor patterns, political goals, and social ideals. Similarly, the business community was badly divided—between conservative Democrats and radical Republicans, between older patrician merchants and newer rising industrialists, and between free traders and protectionists. They differed over protection of the city's black community, how to control or reform the working class, and the nature of social justice and political authority. By 1863, Bernstein writes, "assertive workers faced an elite profoundly divided over the most basic questions of social and political rule" (p. 191). Consequently, the Lincoln administration's enforcement of the draft "provoked a riot and a crisis in New York because it crystallized and gave sudden focus to these mid-century urban disputes" (p. 191).

The book is not without problems. The white working class and the business and political elites are covered in great detail, but the city's black community is barely explored. In the early chapter narrating the riots, Bernstein tends to speculate often about motivation in the absence of hard evidence. In little more than a dozen pages, the author uses on numerous occasions such phrases as "probably," "presumably," "no doubt," "may have," "may well have," "seems to have been," "more likely than not," "seem to have imagined," "may have speculated," "reason to suspect," and the like. For instance, violence against blacks "may have been" a form of "sexual policing" (p. 31), a psychological interpretation for which there is little convincing evidence. This tendency toward speculation undermines the analysis at first, but the rest of the book quickly becomes more authoritative and more persuasive. Along with recent studies of nineteenth-century New York City by Sean Wilentz, Amy Bridges, Christine Stansell, and Elizabeth Blackmar, this superb book will surely reshape the parameters of mid-century urban and social history.

RAYMOND A. MOHL
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EARL M. MALTZ. *Civil Rights, the Constitution, and Congress, 1863–1869*. Lawrence: University Press of Kansas. 1990. Pp. xiii, 198. \$25.00.

Earl M. Maltz offers a provocative account of the origins of the Thirteenth, Fourteenth, and Fifteenth amendments to the Constitution. Because the author believes the courts should be "bound by the original understanding" of legislative enactments, his narrow

reading of the lawmakers' intentions has significant contemporary implications (p. ix).

In examining the ideas of Reconstruction-era legislators, Maltz distinguishes between a commitment to "total racial equality," which had marginal appeal, and "limited absolute equality," the mainstream Republican position that "all men were equally entitled to a limited set of natural rights" (p. 4). Moderate and conservative Republican legislators found the latter view congenial. Such men opposed unlimited expansion of federal power, especially over private citizens, and the result was "a series of enactments that reflected the ideology of conservative Republicans rather than their more radical compatriots" (p. 157). Maltz contends that the legislative intent of the moderates should receive special emphasis, and if this is "accepted as the appropriate measure of the original understanding," then the Thirteenth Amendment should be construed in a restrictive fashion (p. 27). Moderates were in a position to demand the "narrowest possible interpretation" as a condition for their support, and even in the absence of any such formal demand, Maltz argues that it should be inferred (p. 24).

Similar arguments recur with respect to other legislation, but such interpretations seem strained. Should the views of a few "swing" legislators outweigh the views of most of their Republican colleagues? In the case of the Thirteenth Amendment, Maltz oversimplifies a political situation in which lame-duck Congressmen realized that emancipation would be sanctioned by the incoming Congress. In a period of intricate factional maneuvering, with the Democrats often voting for tactical reasons with the Radicals, Maltz may exaggerate the determining influence of "moderate conservatives."

If overall Maltz defends a narrow reading of Reconstruction statutes, then his occasional departures from that view become all the more interesting. His position on the hotly debated Fourteenth Amendment "incorporation" issue is striking. The author concludes that "contemporaries must have understood the privileges and immunities clause to embody most of the Bill of Rights, and they probably viewed the first eight amendments as incorporated in their entirety" (p. 118). The contrary interpretation of Edwin Meese and other legal conservatives receives little support here.

A few caveats should be noted. Ending the book in 1869 is perhaps a bit misleading. In the early 1870s, Congress relied on the Reconstruction amendments to pass expansive civil rights legislation affecting individuals rather than states. Few Republican congressmen doubted their authority to do so, which is surely relevant to their understanding of the earlier legislation. Furthermore, Maltz also slights the impact of the Reconstruction amendments on women. Ellen Carol DuBois's seminal work bearing on this topic does not appear in the bibliography.

Despite such objections, Maltz's book is of consid-

erable interest as a challenging interpretation of the era. It is both concise and consistent and should be accessible to a wide range of scholars.

MICHAEL W. FITZGERALD
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DAVID M. GOLD. *The Shaping of Nineteenth-Century Law: John Appleton and Responsible Individualism*. Foreword by MICHAEL LES BENEDICT. (Contributions in Legal Studies, number 57.) New York: Greenwood. 1990. Pp. xiv, 229. \$42.95.

John Appleton is hardly a household name, even among American legal historians. Yet David M. Gold's analytical biography of the former chief justice of the Maine Supreme Court not only rescues Appleton from obscurity but also informs current debates about the nature of nineteenth-century legal culture.

Appleton served on the Maine court from 1852 to 1883, attaining the position of chief justice in 1862. His name appeared as advocate or judge in sixty-eight volumes of the Maine reports, and he successfully lobbied the state legislature to bring about needed legal reforms, a reorganization in the state judiciary, and higher standards of practice. Gold acknowledges that Appleton was not a great common law judge in the mold of Lemuel Shaw, but shows that his influence nonetheless extended well beyond Maine. His most important contributions came in the area of evidence, and Gold argues persuasively that "There is no doubt that Appleton deserves much of the credit for the great nineteenth-century reforms in evidence law" (p. 167). Appleton, for example, was the first American jurist to urge the adoption of the parties as evidence rule, and he successfully pressed the Maine legislature in 1856 to enact the first statute making parties in civil suits competent to testify.

The strength of Gold's book derives from his ability to frame Appleton's achievements within current scholarly debates. Whatever their political predilections, most students of nineteenth-century law have stressed the significance of economic interests. Gold, however, turns to ideas and cultural patterns to explain Appleton's behavior, borrowing from Roscoe Pound's argument that a "taught tradition" of law animated judges rather than crass economic considerations. In the case of Appleton, this "taught tradition" emphasized the New Englander's insistence on self-reliant individualism and the reciprocal concept of personal responsibility. Gold's most original contribution is to argue that laissez-faire individualism did not entail social atomization and isolation. Instead, Appleton's career demonstrated repeatedly that a free society required the rigid enforcement of rules of personal accountability (whether through tort, commercial, constitutional, or contract law) for one's conduct toward other people.

Appleton played out his life on such a limited canvas that his biography can hardly provide a secure

base from which to generalize about the connections among cultural beliefs, legal thought, and the law in action. Still, Gold's book does strengthen the arguments made by his mentor, Michael Les Benedict, that legal historians should not confuse nineteenth-century self-reliance with twentieth-century nonconformity. Victorian-era notions of individual freedom carried with them the requirement for responsible social behavior; individual liberty, according to Gold, was a far more subtle concept than proponents of laissez-faire constitutionalism and "conservative crisis" have appreciated. In sum, this is a solid biography that contributes directly to the debate over the relative importance of economic interests and ideas in the nation's legal past.

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LEWIS O. SAUM. *The Popular Mood of America, 1860-1890*. Lincoln: University of Nebraska Press. 1990. Pp. 284. \$40.00.

Lewis O. Saum draws on extensive research in letters and diaries to trace how the inarticulate many, whose antebellum outlook had little affinity with that of the articulate few, jettisoned an earlier piety and belief in providential determination for a modernity of spontaneity and naturalistic explanation. As Saum tells the story, the transformation followed directly from the Civil War and western expansion. Sophisticates may have marveled at the order and coordination that made possible both events, but rural whites, who are the subject of this study, discerned only chaos, pointlessness, and illusion.

These experiences, Saum contends, promoted an "enterprising and worldly spirit" (p. 203) that allowed ordinary Americans to enter the twentieth century. By 1890, they no longer fathomed the unfolding of God's plan in their fate on the battlefield or in western ventures and came to regard religion as "no more than an aspect of existence" (p. 77). Freed from the restrictiveness of their religious world view, they pressed the private claims of the self in undeniably earthy terms and demanded that social and political institutions respond to these new desires.

That the late nineteenth century was a period of secularization is hardly a novel conclusion, but Saum adds some intriguing facets. His discussion of the changing meaning of such words as "privilege," "dull," and "satisfaction" is subtle and rewarding. So too are his delineation of commonly held conceptions of society, his argument that the celebration of Christmas emanated from social rather than religious impulses, and his demonstration that the liberal myth of the efficacy of education thoroughly permeated the popular consciousness by the latter third of the century.

With these contributions come shortcomings. Al-

though the occasionally turgid prose, awkward juxtapositions, and cryptic references to other works are disconcerting, the most problematic aspect of the book is the conceptual weakness of Saum's organizing principle, the "common people." Because Saum regards the term as equivalent to the median condition, he excludes from consideration all that is statistically unusual. His limited formulation does not facilitate a full account of the historically specific relations and interests that differentially shaped experiences. Having explicitly exempted gender and race, he implicitly does the same with class and place, factors that one would have thought had some bearing on the concept. Yet the changing rhythms of work and the uneven development of the countryside are not mentioned. Consequently, Saum's portrayal replicates the rootlessness and atomization of the masses found in modernization theory. His subjects reject collective organizations that propose the reconstruction of social conditions. It is surprising in a book that announces a new interest in politics among ordinary Americans to find only a single page devoted to the Grange, a half a page to unions, and none to the Ku Klux Klan, all of which presumably had some "common" members. For all the attention to their achievements, Saum's "humble" Americans prove to be static, passive abstractions rather than historical actors.

DANIEL H. BORUS
University of Rochester

NORMAN POLLACK. *The Humane Economy: Populism, Capitalism, and Democracy*. New Brunswick, N.J.: Rutgers University Press. 1990. Pp. xiv, 215. \$40.00.

After more than thirty years' immersion in the primary sources of late nineteenth-century American Populism, Norman Pollack now presents an extended essay on the Populists' economic ideas, especially their "construction of democratic capitalism" (p. x). In this book he revises his position in *The Populist Response to Industrial America* (1962) by insisting at many points that the Populists were firmly within the capitalist spectrum. Their quarrel, he says, was not with capitalism because they fundamentally assumed the legitimacy of property rights. Their quarrel was with monopolistic capitalism, which the railroads and national banks had pioneered and which was spreading all around them. Moreover, by requiring that government be responsible to the people and independent of corporations, Populists should not now be considered the precursors of the corporate kind of liberalism espoused and accepted by the Progressives a few years later and by Americans generally in the twentieth century.

The book is not a conventional history but verges on political-economic theory. Its cited sources are limited and not usually linked to contexts of time and place. It does not inquire systematically into the origins of the Populists' thought other than to point

out that they drew, with some revision, on John Locke and Thomas Jefferson. No mention is made of their debt to the greenbackers, Henry C. Carey, or others. Yet the author is at pains to state what a variety of Populists, southern or midwestern, editors or office seekers, held in common and where they differed. In this respect he presents a considerably less monolithic portrait of them, one more expressive of nuances and differences than in his 1962 book. His other recent book, *The Just Polity* (1987), on the Populists' political views, possesses the same virtuous improvement. One wonders, in fact, why Pollack did not publish *Just Polity* and *Humane Economy* between the same two covers.

The book contains many valuable passages. "The moral sources of authority" (pp. 50 and following) states what Populists had in common and where they differed (based on his reading of William A. Peffer, James B. Weaver, W. Scott Morgan, James H. Davis, Lorenzo D. Lewelling, Stephen McLallin, Tom Watson, Ignatius Donnelly, and Thomas L. Nugent). The sources he chooses to cite are almost entirely limited to these men, and in certain chapters even they disappear. One is tempted to say that the book is not so much the Populists' composite view of political economy as Pollack's idea of what they would have said and written had they (with his help) been more explicit and consistent—and less caught up in life's daily exigencies. Nonetheless, I cannot say that he has done violence to their thought. Indeed, he has delineated with precision just where they parted company with the controlling, emerging corporate-liberal and monopolistic versions of capitalism. Their views are put forward calmly; one begins to wonder why they were so roundly condemned and hated until Pollack correctly indicates their lower-middle-class, small-holding position vis-à-vis more elevated bourgeois groups (especially on p. 104). Chapter 4 in general explains their class position clearly, how it generated their responses, and what constituted the left, center, and right wings of Populism itself.

Pollack insists correctly that the Populists were not radical (that is, anticapitalist). One of his best insights, which explains much about their theoretical flaws, moral admirability, and electoral failure, is that "Populists did not think in ideological terms; they thought in ethical terms, with added pragmatism" (p. 112). Did they offer an alternative view, not only to laissez-faire, monopolistic capitalism, but also to most of twentieth-century reform? Assuredly. He concludes, "They approached the border between radicalism and reform: a position of authentic reform, which would establish democratic capitalism. They transcended liberalism. . . . Populism was more American than America had any right to expect" (pp. 168–69). Nicely said.

WALTER NUGENT
University of Notre Dame

ALLEN W. BATTEAU. *The Invention of Appalachia*. (The Anthropology of Form and Meaning.) Tucson: University of Arizona Press. 1990. Pp. viii, 239. \$29.95.

Written in the tradition of scholarship in American studies, this work analyzes the images that a long line of Americans have constructed to describe the people of Appalachia. It reveals both the strengths and the challenge of an American studies approach. Its strengths lay in its intricate depictions of the various images of Appalachia. Anthropologist Allen W. Batteau asks why the rest of America (or certain parts of it) needs an Appalachia, and how those needs have changed. In this effort, the book is a success. Arguing forcefully that "at bottom, all consciousness is poetic" (p. 8), Batteau offers no apologies for studying images rather than the structure of social life. The challenge of the work is to address issues of power relationships embedded in those images. Have the images worked to hinder mountaineers' efforts at political mobilization or labor protest? In answering that question the work is provocative, but not quite as successful.

The volume's importance lies in the number and variety of its analyses. Rather than pointing toward a single argument, the author builds insight on insight, with descriptions that are at their best when they are at their thickest. Appalachia has usually represented something that the dominating culture has either sadly left behind or proudly overcome. Many of the images are related to something natural and somehow pure. In her fiction, Mary Noailles Murfree associated mountain people with domestic life, femininity, and folk traditions. In doing so, she set a precedent by joining these people to institutions that were losing real power in society while gaining an air of inherent virtue. Beginning in the 1880s, writer John Fox and educator William G. Frost best exemplified movements that viewed mountain people as backward, uncivilized representatives of an Anglo-Saxon race that many considered under siege. The author makes some of his best points in discussing the handcraft traditions sustained by Frost's students at Berea College. In an age of increasing mass consumption, many Americans desired a "coarseness and individuality" and a "spontaneity and personality machined goods lack" (p. 80). In another imaginative section, Batteau shows that many American interpreters of the region portrayed the labor strikes of the 1920s and 1930s as folksy curiosities and the strikers as sad victims of the nation's harshest form of exploitation. The mountaineers' protests thus seemed unique to the area and not part of any broader movement of the American lower class.

Later sections of the book show that images of the region have grown more negative since the 1930s. Americans believing in the New Deal ideal of a scientifically planned society saw in the dirt and disorder of mountain life a sign of the "backwardness" that resulted from a lack of organized planning.

Extended descriptions of John F. Kennedy's West Virginia campaign and a documentary hosted by Charles Kuralt and televised in 1964 both show that modern images have generally portrayed the mountain people as victims who are helpless without the largess of the larger society. Throughout the book, Batteau's analyses are inspired and ingenious, if not always as thorough as readers might like.

What the book lacks, given its attempt to study both images and their political meanings, is attention to the details of the economic and political forces that have dominated Appalachia. In saying that Americans have constructed ideas of Appalachia to suit different needs, Batteau could do more to show how those ideas have influenced policy. He argues persuasively that images and power relationships are "different faces of common, underlying social processes" (p. 199), and therefore progresses beyond works that try either to debunk myths for "reality" or to reveal the cynical uses different groups have had for the mountains. He makes promising beginnings by arguing that policy makers must take into consideration the long history of images of the region when evaluating their own motives, and by claiming that mountain residents have appropriated many of the images in their dealings with policy makers. Still, the book could serve both scholarship and the interests of the mountain residents better if its analysis of the relationship between images and power equalled the complexity of its analysis of the images themselves.

TED OWNBY
University of Mississippi

WILLIAM F. HARTFORD. *Working People of Holyoke: Class and Ethnicity in a Massachusetts Mill Town, 1850-1960*. (Class and Culture.) New Brunswick, N.J.: Rutgers University Press. 1990. Pp. xii, 294. \$42.00.

This fine study of a medium-size paper and textile manufacturing city in western Massachusetts concentrates on the interplay of ethnicity and class within the local Irish community. Although Holyoke had other smaller groups of immigrant factory workers, William F. Hartford focuses primarily on tensions between the more numerous Irish and French Canadians and the dominant Yankees who controlled Holyoke's wealth and power. Many ethnic women also worked in the local industries and, as weavers and paper finishers, led episodes of early twentieth-century labor protest. Yet there is little analysis of gender relations either at work, in the politics of labor protest, or within the ethnic enclave. Despite these limitations, Hartford's study is a welcome addition to community studies in the industrial northeast.

Hartford investigates how ethnicity shaped Irish-American working-class experience by examining conflicts within the Irish enclave between skilled workers and the middle class. Irish laborers in the 1850s were championed by the city's first Catholic

pastor who drew on Irish antilandlordism. This promising situation passed, however, when protesting workers were drawn off into a saloon culture and succeeding priests concentrated on building churches and enclave respectability. Thus, the Catholic church, allied with the Irish middle class and its Democratic party activists, created the respectable Irish working class. Through parochial schools and religious training, through the promotion of stable family life, temperance, and thrift, the church nurtured enclave respectables who later became the leaders of trade unions. Yet this excluded the unlucky, unskilled, and rebellious, thereby dividing respectable Irish workers from other ethnic groups in the lowest paying industrial jobs. Enclave consciousness began to dissolve by 1890 when skilled construction workers struck Irish-owned companies, but respectability and craft allegiance continued to limit the Central Labor Union's brand of class consciousness. One fascinating chapter analyzes the conflicted relationship in the early twentieth century between the church and the trade unionists.

There is some tension between Hartford's overly schematized introduction (enclave consciousness, 1850–90, and class consciousness, 1890–1960) and the suggestive richness of the detailed chapters. Why is the undermining of class resentment during the enclave period, through ethnic conflict, craft exclusiveness, and paternalist employer practices, characterized as “class harmony” (and thus enclave consciousness) rather than class hegemony (p. 36)? The focus on the clash of Irish politics seems, furthermore, to obscure the political dynamics within the factories and mills and within domestic life that underlay the trade unionists' decision to support the family wage. Was there a connection between rising levels of militancy after 1898 among Irish, French Canadian, and Polish women workers and decisions made by the Irish trade union respectables and the enclave-promoting church to embrace the family wage? Did they fear losing control over the next generation of young Catholic men and women? Hartford rightly deplores as crucial losses to labor politics clerical antiradicalism and the Irish failure to grant French Canadians leadership of textile unions, but the reasons remain obscure. Still, skillful community studies like this provide the best opportunities to pursue such analysis.

MARY H. BLEWETT
University of Massachusetts,
Lowell

ILEEN A. DEVAULT, *Sons and Daughters of Labor: Class and Clerical Work in Turn-of-the-Century Pittsburgh*. Ithaca: Cornell University Press. 1991. Pp. xii, 194. \$21.50.

In this book, Ileen A. DeVault demonstrates the continued saliency of community studies for illumi-

nating broad theoretical questions. Analyzing the backgrounds, aspirations, and occupational trajectories of students enrolled in the Pittsburgh high school Commercial Department at the turn of the century, DeVault traces the shifting and permeable boundary between blue-collar and white-collar workers. She examines as well the changing social and economic context into which commercial department students moved and delineates the way increasing stratification within the service sector was obscured by its concomitant feminization.

This superb study combines detailed quantitative analysis with provocative life histories. DeVault explores general shifts in educational and occupational patterns alongside individual and familial decisions, which both responded and contributed to those shifts. She thus reminds us that the force and direction of historical change, so clear in retrospect, is often experienced by those in its midst as a set of confusing options with uncertain outcomes.

The sons and daughters of unskilled laborers, skilled laborers, clerical and sales workers, and proprietors all attended the Commercial Department, yet their experiences in both the classroom and the workplace were quite distinct. In tracing the meaning of these experiences, DeVault charts changes in the neighborhoods from which commercial students and white-collar workers emerged. She argues that some groups, such as the skilled laborers living in Hill Top, absorbed white-collar children into old community structures, while others, such as those in Lawrenceville, found in clerical work a way of distinguishing themselves from new immigrant neighbors. She then shifts our attention back to the workplace, illustrating the ways that the white-collar labor market, like Hill Top, could absorb working-class sons and daughters without itself being transformed.

Women, moreover, could enter white-collar work without dramatically altering the dynamics of gender at work or at home. Certainly offices provided more heterosocial worlds than most factories, but in general sex role expectations shaped these workplaces more than they were reshaped by them. Indeed, despite the mingling of sons and daughters in the Commercial Department and the office, differing expectations, dissimilar job markets, and distinctions in pay and promotions marked the educational and occupational experiences of women and men.

Finally, DeVault examines those families in which some children were employed as clerical workers and others as skilled laborers, reminding us that “during the decades of clerical work's efflorescence, the actions of workers were not necessarily predicated on assumptions of white-collar superiority” (p. 176). The “collar line,” she concludes, “was not so much a social chasm as it was a social estuary, a site for the mingling of economic groups and social influences” (p. 177). By charting the ambiguities of those who entered this estuary for the first time, DeVault makes us acutely aware of the complexities of life choices at the turn of

the century and warns us, implicitly, against fabricating in hindsight ordered and self-conscious images of change that belie the experiences of those whose histories we seek to resurrect.

NANCY A. HEWITT
University of South Florida

DANIEL B. CORNFIELD. *Becoming a Mighty Voice: Conflict and Change in the United Furniture Workers of America*. New York: Russell Sage Foundation. 1989. Pp. xii, 292. \$35.00.

The blurring of disciplinary lines has been a scholarly trend of some note during the past two decades. Nowhere has this been more true than in sociology, where numerous works have joined Barrington Moore, Jr.'s pathbreaking *Social Origins of Dictatorship and Democracy: Lord and Peasant in the Making of the Modern World* (1966). Daniel B. Cornfield's book, whose subject is the United Furniture Workers of America (UFWA) from its founding in 1937 to its merger with the International Union of Electronic, Electrical, Technical, Salaried, and Machine Workers (IUE) in 1987, is one such work of historical sociology.

Cornfield has chosen historical material for his subject, but his problem is pure sociology. His goal is to test competing theories of union leadership change: Robert Michels's "iron law of oligarchy" (*Political Parties* [1915]) and its two alternatives, "structuralist" analysis and "resource mobilization theory." All three theories, he argues, offer inadequate explanation when tested against the historical data. He instead proposes a "status conflict theory" that takes into account both structure and agency as well as internal and external developments.

Historians, especially labor historians, will find little of interest or exception in Cornfield's criticism of the predominant theories of union leadership change. The main strength of his status conflict theory is its emphasis on contingency, which for most historians is not an issue. That he has cast his alternative analysis in hypotheses, liberally sprinkling it with "mays," suggests how contingent even his "theory" is.

The historical sections of this study, however, are important. Comprising about three-quarters of the book, this examination of leadership change in the UFWA is the best and most complete I know of for any twentieth-century union. Through an intensive analysis of changing leadership patterns on the UFWA's General Executive Board and their relationship to both the larger political economy and internal union politics, Cornfield makes several important points. First, the size of the union local, when it was founded, its geographical location, and its social composition were of critical importance for internal union dynamics and changing leadership patterns. Second, the UFWA's stable period from 1950 to 1970

lends but superficial credence to Michels's "iron law": capital mobility in the furniture industry posed extraordinarily difficult problems, to which the leadership tirelessly but unsuccessfully tried to find solutions. This period, moreover, saw the union leadership engaged in a "mentoring" process that partially accounts for the emergence of African-American, Hispanic, and female leadership during the late 1960s and 1970s. The irony is that this concurred with the union's period of decline, when its membership dropped from 30,690 in 1970 to 19,367 in 1985.

Yet another strength of Cornfield's work is that he shows how influential UFWA's left-wing origins were. While anticommunists defeated the communist Left in the 1950s, thus saving the UFWA from CIO expulsion, the ideology of left-wing trade unionism continued to predominate, affecting UFWA's history both deeply and positively. The old guard in general welcomed and nurtured the new arrivals to leadership during the 1970s and 1980s.

Historians will learn much, both in content and issues raised, from the historical sections of this book, though little from the sociological ones. Wading through the jargonistic writing and the seemingly endless "as discussed earlier" formulations is worth the effort.

STEVE ROSSWURM
Lake Forest College

CLARK C. SPENCE. *The Conrey Placer Mining Company: A Pioneer Gold-Dredging Enterprise in Montana, 1897-1922*. Helena: Montana Historical Society; distributed by University of Washington Press, Seattle. 1989. Pp. x, 161. \$19.95.

Clark C. Spence has shed new light on a hitherto largely ignored phase of western mining by ransacking scattered sources to piece together the story of the Conrey Placer Mining Company. Although its gold-dredging operation along Alder Gulch, about sixty miles south of Butte, Montana, was geographically remote, Conrey was very much in the center of early development of dredging technology. Spence's account reaffirms a now familiar story of technological innovation by trial and error and dissemination of ideas within a small international circle held together by a common interest. Borrowing from experience gained in harbor and canal construction and earlier dredge mining in New Zealand and nearby Grasshopper Creek, Conrey managers adapted their machinery to the unique conditions of their Montana properties. Spence maintains that geographic isolation caused the company to lag in the continuing use of new technology, and innovation passed to California, which became the center for the application and spread of new dredging ideas.

The Conrey mine also provides an example of a business organization that Spence describes as "an

unusual combination, guided by unusual people" (p. 17). From its beginning the company had access to substantial Boston capitalists such as millionaire Gordon McKay, whose bequest to Harvard College conveyed a controlling share of the company. With such backing Conrey was not a typical small mining operation perpetually short on cash and dependent on promoters to drum up capital. Among investors were also knowledgeable men such as Philip N. Moore, mining consultant; Nathaniel S. Shaler, noted Harvard geologist; and Hennen Jennings, internationally known mining engineer. Spence demonstrates that organization, financing, and management of even so relatively small an enterprise was complicated by reorganizations and the use of satellite companies to acquire real estate.

Boston investors did not try to dictate the day-to-day operations and relied on decisions by local supervisors who had come up through company ranks. This system appears to have increased returns on investment and may also have aided in establishing generally good relations between management and workers. The remoteness of the town of Ruby created problems (such as alcohol abuse) that were only partially alleviated by baseball and basketball teams and other recreation. With its favorable working conditions, Butte was close enough to stir local demands for an eight-hour day, which the company granted along with bonuses. Although not so hazardous as underground mining, dredging had its own dangers. Work was regularly punctuated by accidents and death. Company officials blamed mounting labor problems during World War I on agitation by the Industrial Workers of the World.

Until its liquidation in 1922, Conrey made money for its investors. But at what cost? As a mass production form of placer mining, dredging wreaked havoc on the land. Spence avoids any extended discussion of dredging's environmental impact by noting that, in Montana at least, few people were bothered by the rubble that dredges left behind. Local citizens were more likely to view dredges romantically as akin to Mississippi River boats—and to accept Main Street profits from development—than to express concern about devastation of the land.

RICHARD B. ROEDER
Carroll College

PAUL J. MIRANTI, JR. *Accountancy Comes of Age: The Development of an American Profession, 1886–1940*. Chapel Hill: University of North Carolina Press, 1990. Pp. xi, 275. \$29.95.

Modern U.S. political history has been made not only by the struggles of parties and mass movements but also by the less visible efforts of technical professions to secure for themselves a claim to social management. In the process, the new professions redefined both themselves and their society. Paul J. Miranti, Jr.,

extends this point to encompass public accountancy and in doing so provides us with the new standard on the subject.

Accounting has had its historians, most recently Gary John Previts and Barbara Dubis Merino. Miranti, however, brings to his study a greater appreciation of the recent work of organizational historians and sociologists of the professions. Most sociologists of professions emphasize the traditional fields of law, medicine, and engineering, and understandably Miranti chooses to compare accounting to these professions. But in its temporal origins and technical pretensions, modern accounting perhaps is more revealingly similar to such social sciences as economics and sociology, and it is regrettable that the book does not consider these fields. Miranti builds much more successfully on the political economy pioneered by William A. Williams, Louis Galambos, Ellis Hawley, and Barry Karl. Like them, he emphasizes the associational ideologies and tensions within and among emergent government, business, and professional elites.

Accounting's modern story is one of constant struggle to achieve internal consensus and social sanction. Emerging in mid-nineteenth-century Britain, it soon flourished in the United States due to business, government, and investor demands for objective economic data. Early on, however, the profession divided along class, ethnic, and ideological lines. A core group of elite practitioners sought to emulate the tradition of British chartered accountancy with its emphasis on social class, family connections, and apprenticeship. Other groups challenged this exclusivity, offering a meritocratic and modernizing vision more attuned to the competitive needs of ordinary practitioners.

Early twentieth-century insurance scandals and the rise of federal regulation gave accountants new opportunities both to demonstrate their social utility and to move tentatively toward consensus. As the profession nationalized, however, elite practitioners still held uneasy sway over accounting's "little men" (p. 84). A new professional journal, annual meetings, and university training promoted a professional vision but not a unified profession. Still, World War I's associational mobilization elevated accountancy, as it did other professions, into a new and more national relationship with managerial leaders. The war's certification of the public authority of private groups helped to promote a postwar role for accounting as a respected part of the New Era's associationalism.

Stricter qualifying examinations and ethical standards nonetheless were frustrated by the accountants' continuing inability to surmount factionalism. The stock market's collapse only worsened matters by challenging the accountants' pretensions to accuracy and disinterestedness while setting the stage for the New Deal's Securities Acts of 1933 and 1934. These and other measures, Miranti concludes, intensified associationalism by establishing a role for accountants

and other professional groups in modern macroeconomic management, created for accountants their modern functional identity, and finally brought them an elusive unity.

GUY ALCHON
University of Delaware

DEBORAH FITZGERALD. *The Business of Breeding: Hybrid Corn in Illinois, 1890–1940*. Ithaca: Cornell University Press. 1990. Pp. xi, 247. \$29.95.

Deborah Fitzgerald is guided, she says, by two questions: how pure science becomes applied science, and how science at land-grant universities differs from science in private agribusiness. The story of hybrid corn is her vehicle for pursuing these questions. She discovers that the traditional differentiation between pure science in universities and applied science in companies was by no means so definite.

Before the business of breeding, farmers practiced selection, the folk method of seed saving and improvement. Even as conscious efforts at corn breeding got underway, they were fairly simple. "Anybody can cross corn," Henry A. Wallace said. Neither did Mendelian genetics magically transform corn breeding, for even within the federal Bureau of Plant Industry, C. P. Hartley remained committed to selection and to homely extension work with boys' clubs—this despite Donald F. Jones's breakthrough in hybridization, the double cross method.

Scientific corn breeding and hybridization proved potent, however, because they benefited from "a peculiar juncture of pure and applied science" (p. 42). College botanists took up corn breeding in order to work out Mendelian principles, while private breeders labored for profit. This juncture was uneasy. Funk Brothers, a major seed company, developed cozy relations with the U.S. Department of Agriculture (USDA), while in the field agricultural colleges and seed companies vied for control of "the machine" of corn breeding and for the "attention and respect" of farmers. As Fitzgerald weighs the matter, the seed houses won: "No longer just seed houses, they became dispensers of scientific knowledge" (p. 214).

Fitzgerald's book is a splendid contribution, splendid for its sound research, its clear exposition, and particularly for its conception. Its conclusions cannot be gainsaid; it will endure as a standard work.

The book may not be quite so pathbreaking as the author claims, however. She asserts that "agricultural historians have neglected the relationship between science, technology, and agriculture" (p. 1) and that historians of science and technology likewise have neglected agriculture. This is hardly the case. Agricultural historians, in fact, often seem obsessed with science and technology, to the exclusion of matters cultural. Most early work along these lines—much like early corn breeding—was unsophisticated (A. C.

True comes to mind), and generally USDA-whiggish, but today it is easy to assemble a bibliography of historians including such names as Earl Hayter, Margaret W. Rossiter, Ronald Tobey, Pete Daniel, Alan Marcus, and Doug Hurt (for a casual beginning), documenting sustained attention to agricultural science and technology.

Finally, it may be helpful to add that in the business of corn breeding, as in agricultural science broadly defined, there is one powerful agent not accounted for by Fitzgerald. She does an excellent job defining the roles of public agencies, agribusiness, and farmers (at least as represented by farm organizations), but this leaves unanswered questions such as why hybrid corn should be developed and promoted at a time when farmers were piling up grain surpluses. The missing agent here is the public interest, or the perception of the public interest, the force behind much of American agricultural policy from the Progressive era through most of this century.

THOMAS D. ISERN
Emporia State University

ROGER DANIELS. *Asian America: Chinese and Japanese in the United States since 1850*. Seattle: University of Washington Press. 1988. Pp. xviii, 384. \$24.95.

Roger Daniels, well-known for his definitive studies of Japanese in America, has written what he calls a "synthesis" that covers the spectrum of the Chinese and Japanese experience in the United States. It is at once both a textbook and an interpretive history. One of Daniels's main arguments is that the Chinese and Japanese immigrants were not unique but rather shared many characteristics with other immigrants. Some important similarities are that the vast majority of immigrants were sojourners who came to the United States for economic betterment, Europeans working in industry on the east coast and Asians working in mining and agriculture on the west coast; most entered unskilled occupations with low pay and status or worked in the service industry within their ethnic community; the squalor of Chinatowns was no worse than some immigrant neighborhoods of large east coast cities; the birth rate of Japanese was at or below the rates of several groups of European immigrants; and most second-generation Asian immigrants came to share the same dream that infused second-generation Europeans in America.

There are also differences: Asian-American history has been largely viewed in an indirect fashion (Daniels uses the term "negative"), focusing more on what has been done to them rather than what they have accomplished; most Asians did return to their country of origin (unlike Europeans); immigration from Asia tended to involve the United States in diplomatic imbroglios; the unfair treatment of Asians tended to discredit American democracy; and Asian immigration rates were minuscule, with more Italians enter-

ing the United States in one year (1913) than the total number of Japanese who entered before 1924.

Daniels's discussion of the Chinese includes some notable insights. He states that the Chinese generally failed to acculturate not because of their inability to do so but rather because the United States placed barriers in their way. The Chinese Exclusion Act of 1882, by effectively preventing women from immigrating, produced an aging bachelor society. As a result, generational conflicts were always won by the older generation, which reinforced conservatism and retarded the process of Americanization. While this interpretation suggests that discrimination was the cause and lack of acculturation the result, it may also be possible, as some scholars have suggested, that discrimination may have resulted from successful efforts at acculturation. The common thread, of course, was the argument by exclusionists that Chinese were unassimilable. Exclusion was also class-based and racially based, that is, employers always wanted Chinese while workers and their unions did not, fearing wage and job competition. And most Americans were accustomed to discriminating against people of color, as the African-American experience testifies.

Japanese came later to the United States and their history was intertwined with Japan's great-power status and its desire to maintain its prestige in the Western world, a posture that the Chinese were unable to adopt. Thus, discrimination against Japanese had to be carried out in a much more delicate manner. There could be no Japanese Exclusion Act, for example, and the Gentlemen's Agreement "solved" the delicate matter of limiting Japanese immigration in the first decade of the twentieth century. This agreement did allow for picture brides to enter, making the Japanese community more evenly balanced between males and females and allowing for the development of a more U.S.-oriented *nisei* (second generation) population that naturally acculturated at a faster rate than the Chinese. Daniels agrees with my findings and those of Yuji Ichioka that the Japanese government was much more solicitous of Japan's image in the Western world than it was with protecting its immigrants abroad. Thus, it emphasized conformity to American habits and values and counseled academic excellence, which left a legacy of acculturation for subsequent generations. The Japanese American Citizens League (JACL) adopted a similar pattern. Daniels concludes that the JACL was successful in defusing anti-Japanese feelings after the war, but this came at the price of silencing those Japanese Americans who denounced the internment camps as undemocratic and un-American.

The rapid upward mobility of Asians, especially Japanese, occupies Daniels's concluding chapter. Are Asian Americans model minorities? Daniels acknowledges the success that Asians have attained, but argues that these successes should not be used politically to criticize other minority groups that have not

done so well—"blaming the victim," as he would term it. Not all Asians have done well, and certainly there are successful African Americans and Chicanos. Moreover, Asians still suffer from discrimination. He concludes that neither the stereotype of model minority nor the opprobrium of "scapegoat" should be used. Although it is not clear to me what he means by scapegoat, it is clear that he rejects the model-minority thesis.

Daniels demonstrates a masterful grasp of the literature. His references to European immigrants add a comparative richness to the volume. As one would expect, his section on the Japanese before and during World War II is the most extensive part of the book. Fortunately, Daniels includes primary sources throughout the work rather than simply relying on secondary works, which one might expect in an overview of this type. Indeed, the historiography in the footnotes is by itself worth the price of the book. An edition in paperback probably would guarantee that this work will become the standard text in Asian-American history courses.

WAYNE PATTERSON
St. Norbert College

EGAL FELDMAN. *Dual Destinies: The Jewish Encounter with Protestant America*. Urbana: University of Illinois Press. 1990. Pp. xi, 339. \$34.95.

Egal Feldman has written a useful survey of Protestant-Jewish relations that is suitable for the general reading public and undergraduate students in American studies, American history, American ethnicity, and American religion. In adopting a primarily Protestant perspective, Feldman tells us what historic assumptions Protestants have made and still make about Jews, how selected community members and groups activated these assumptions from the colonial period to 1990, and how American Jews have reacted to these assumptions as a minority within an overwhelmingly Christian environment.

Culled from a variety of nonarchival sources, which include edited papers, memoirs, biographies, autobiographies, monographs, scholarly articles, and secondary works, Feldman discerns three general Protestant types. The first of these, liberal Protestants, emerged in the eighteenth-century Enlightenment, found expression in the words of Thomas Jefferson, were idealized in Ralph Waldo Emerson's transcendentalist philosophy, were represented by advocates of the Social Gospel during the Progressive era and, by 1941, included such powerful voices as John Haynes Holmes, Harry Emerson Fosdick, and Reinhold Niebuhr. Among all Protestant groups it was this type that heralded the greatest promise for fruitful Protestant-Jewish relations because it bore a close resemblance to traditional Jewish concepts of social activism, social justice reform, and communal betterment. With certain post-1920 exceptions (for exam-

ple, George Foote Moore, Everett Clinchy, Frank Littell, and A. Roy Eckardt), liberal Protestantism failed to capitalize on its promise because it regarded Judaism as particularistic, that is to say, insufficiently "universal," archaic, tribal, materialistic rather than spiritual in a Christian sense, and avaricious.

The second group are Evangelicals who find their roots in Puritan New England, America's two great awakenings, assorted missionary groups of the nineteenth and twentieth centuries, and, most recently, the Moral Majority. These are "Bible" or "Old Testament" Christians who need Jews for the sole purpose of realizing Protestant eschatology and apocalyptic yearnings.

The last and apparently latest blooming of these major groups are the secular Protestants who have adopted nineteenth- and twentieth-century racial attitudes. They view Jews as biological "problems" and as a collective threat to secular Protestantism's perceived hegemony. Some of the more notable names are those of Madison Grant, H. Richard Niebuhr, Burton Hendrick, Henry Ford, and Kenneth Roberts, all of whom were deeply affected by the "Old Stock Panic" of the 1920s.

Despite their diversity on many issues, most of these Protestants have a commonality: a tendency to see the Jewish religion, Zionism, and Jewish civilization as extensions of Christianity rather than dynamic and unique processes that, in fact, have meaning only for Jews. As the author correctly implies, Judaism was and always will be disappointing for Protestants who regard it as a mirror of their own destiny and, invariably, anti-Semitism will result.

Some minor errors in this book should be noted. The accepted year for the destruction of the First Temple is 586 B.C., not 588 B.C. (p. 100). Charles Crane was not a "liberal theologian" (p. 170) but a wealthy Chicago businessman, and his associate on the King-Crane Commission of 1919, Henry Churchill King, is the "liberal theologian" in question; Jesse Helms is not from "South" but from North Carolina (p. 238); and, finally, as an error of omission, liberal response to the Holocaust must include the work of Robert Abzug.

Feldman's book should be commended for reminding readers of a simple, yet profound, truth: in math as in pluralistic, multireligious, multiethnic cultures, the sum of one and one should always be two.

STUART E. KNEE
College of Charleston

SUSAN A. GLENN. *Daughters of the Shtetl: Life and Labor in the Immigrant Generation*. Ithaca: Cornell University Press. 1990. Pp. xii, 312. \$29.95.

Recognition of the differences among women engendered by class, region, sexual orientation, and especially ethnicity and race has made it difficult for American women's historians to produce a unitary

history of their subject. Fearing the possible fragmentation of their field, they are now attempting to produce a multicultural framework that can adequately position diverse groupings of women and analyze the complex interactions among them while giving each unique experience its due. Susan A. Glenn makes a timely contribution to this effort.

Integrating labor, social, and cultural history, Glenn examines the young East European Jewish women who arrived in eastern and midwestern American cities around the turn of the century and found work in the rapidly expanding needle trades. She focuses on their lives during the period between girlhood and marriage, when they entered sweatshops and factories and learned to negotiate between two distinct and often conflicting cultures. Like many contemporary historians of immigration, Glenn eschews a top-down model of Americanization in favor of one that sees assimilation as a process of exchange. She shows that ethnically heterogeneous female work groups turned factories into schools where immigrants absorbed the customs and values of the new world and introduced the native born to radical political ideas.

Through a close analysis of employment patterns, Glenn challenges the notion that immigrant women's jobs in the needle trades were invariably "dead-end." Although "unwritten" gender laws reserved the most lucrative occupations for men, some women were able to work their way up by capitalizing on existing skills or deliberately moving from one low-paid "learner" job to another until they could claim coveted positions as sample makers. Inspired by "Old World artisan traditions," Jewish women's ambition carried them some distance in an industry where "gender boundaries remained somewhat permeable . . . [and] the deskilling process was uneven and incomplete" (p. 130).

Jewish women quickly learned that in America "it was presumed 'ladies' received respectful treatment" (p. 175). Rejecting the assumption that immigrant women's desire for respectability automatically led to their becoming bourgeois, Glenn contends instead that it moved them toward labor militancy. Although blocked from the highest leadership positions, female Jewish activists, along with their Italian counterparts, forced garment workers' unions to respond to the demands of a growing female rank and file by starting educational departments and all-female branches (a move criticized by some for further ghettoizing women).

The years spent in shops, union halls, and political meetings were formative ones for young Jewish women but did not necessarily prepare them for the expected roles of wife and mother. They became, as a result, an "unsettled generation," remaining active in labor and radical politics even after they had given up their jobs to start families. Although contemporaries of the American "new woman," this cohort of Jewish females pursued a different agenda, privileging eth-

nic over gender solidarity. In a surprising reversal of the assimilation process, Glenn argues, it was the immigrant women who served as models for native-born activists.

Although not the first study of this immigrant group, this book turns up new data and offers many fresh insights. By effectively using linguistic methods, Glenn is able to unpack the gender codes of labor and management rhetorics. Her comparison of Jewish and Italian women's participation in labor movements is useful but all too brief; a multicultural understanding of American women will require more extensive systematic comparisons among specific groups. But that understanding also requires sensitivity and balance, and these Glenn supplies in abundance.

SONYA MICHEL
University of Illinois,
Urbana-Champaign

AARON BERMAN. *Nazism, the Jews, and American Zionism, 1933–1948*. Detroit: Wayne State University Press. 1990. Pp. 238. \$34.95.

THOMAS A. KOLSKY. *Jews against Zionism: The American Council for Judaism, 1942–1948*. Philadelphia: Temple University Press. 1990. Pp. xi, 269. \$39.95.

In these two books, Thomas A. Kolsky and Aaron Berman examine the relationship between the Holocaust and Jewish national rebirth and the impact these events had on American Zionism. Although both writers have listed in their bibliographies a substantial variety of manuscript and printed primary sources, the conclusions they arrive at will be challenged by students of recent Jewish history.

Of the two contributions, Kolsky's book, a history of the American Council for Judaism, a Jewish anti-Zionist organization, is the more significant. The Council's initial objective, according to Kolsky, was to preserve a "classical" style of Reform Judaism, one that defined the Jewish experience solely in religious rather than in national terms. Its adherents saw no need for a Jewish return to Zion and were appalled by the growing popularity of the Zionist movement, especially among their own liberal rabbinical colleagues. As their anxieties about Zionism mounted, they discarded their original religious objectives and became by 1943 a secular pressure group dedicated to the destruction of the Jewish national movement.

Although Kolsky's history of the American Council for Judaism constitutes a valuable contribution, its conclusions are not convincing. One problem stems from the author's contention that the Council's ultimate failure stemmed from events not of its own making. If Adolf Hitler's war against the Jews had not intervened, writes Kolsky, the Council would have won the hearts and minds of American Jews, most of whom, he believes, were potential anti-Zionists. The Council's message, he also argues, was "too cold and

rational" (p. 194) to withstand the aggressive tactics and emotional appeal of the Zionists, especially after the horrible revelations of the Holocaust. It could be argued, however, that the Council's inability to prevail in the marketplace of ideas was caused less by the emotional impact of Hitler's extermination policies than it was by the ideas inherent within the Council's message, ideas perceived by most Jews as unreasonable and alien to the traditions and values of Judaism.

Even more questionable is Kolsky's assertion that Zionist preoccupation with building a national homeland and with its war against the Council diverted its focus from the task of rescuing Jews from the Nazi gas chambers. His statement that Zionists "assigned a much higher priority to promoting support for Jewish statehood than to rescuing Jews" (p. 200) reflects not only a serious misunderstanding of Zionist thinking and activities during the tragic years of World War II and the options available to American Jews at that time but also is an example of a tendency in some circles to blame victims for their own misfortune. Such conclusions detract seriously from the value of an otherwise worthwhile monograph.

Even more surprising, Aaron Berman's entire book dilates on the spurious theme of Zionism's neglect of rescue. With considerable repetition, Berman insists that Zionists, despite their profession of concern for Hitler's victims, callously ignored the cries of their European brothers and sisters, or worse, exploited their suffering for the sake of Jewish statehood. Zionists, argues Berman, wedded hopelessly to the "Zionist idea of Jewish history" (pp. 122, 183), were unable to recognize the uniqueness of the Final Solution. This incapacity crippled their ability to respond appropriately to the Holocaust.

Zionists, like all other people, did not anticipate the dimensions of the extermination that the Nazis had planned for the Jews. To suggest that Zionists "found it impossible to give the rescue of European Jewry priority over the creation of a Jewish commonwealth" (p. 122) and that they should have created a "powerful lobby to force the American government to undertake the rescue of European Jewry" (p. 183) is a noble-sounding but misinformed conclusion. It invests the American Zionist movement with the power, influence, and foresight that neither it nor any other American Jewish organization possessed in 1943. Even in the light of the Holocaust, the building of a national homeland for the Jewish people was not as unreasonable an objective as Berman seems to think. Contrary to Berman's opinion, there is every reason to believe that had a Jewish state been founded in 1938 rather than a decade later, millions of Jewish lives would have been saved from Germany's gas chambers.

The book suffers from other misinterpretations. Berman exaggerates the power and influence of American Zionists. Their "propaganda," a term he frequently employs in describing Zionist literature (pp. 89, 178–79), looms large in his imagination. In

addition, he castigates Zionists for their insensitivity toward and suspicion of Arab intentions, whereas he minimizes Arab hostility toward Jews. Even the Mufti of Jerusalem, who was an admirer of Hitler and who, contrary to Berman's denials, endorsed the German leader's treatment of Jews, as did many of his Arab followers, turns out to be not such a bad guy after all in Berman's study (p. 162). But these are minor faults when compared to Berman's indictment of Zionism, the only organization in the world that exerted any effort at all to rescue Jews from Germany's death trap.

EGAL FELDMAN
University of Wisconsin,
Superior

LOREN SCHWENINGER. *Black Property Owners in the South, 1790–1915*. (Blacks in the New World.) Urbana: University of Illinois Press. 1990. Pp. xvii, 426. \$50.00.

Antebellum free blacks, Loren Schweninger argues, were not heaped at the bottom of the southern social scale, nor were propertied blacks the exception after the Civil War. Although Africans usually did not acknowledge private property, they quickly learned that in the New World respectability if not power came with the accumulation of property. Despite restrictions, slaves often owned clothing, household articles, and animals. Given the legal barriers, African Americans acquired an impressive amount of real estate both before and after the Civil War. This book is about industrious and successful people who gained material wealth even as their brothers and sisters suffered under slavery, sharecropping, and discrimination.

Schweninger argues that the system of slavery and the legal repression of free blacks did not close off all avenues for resourceful people to acquire property, even wealth. Before the Civil War, African-American property ownership was greater in the lower South, especially in Louisiana. Sources reveal a great deal about wealthy blacks in this area. For example, in 1830 some fifteen hundred black masters in the lower South owned over seven thousand slaves, 42 percent of all slaves owned by blacks. Carefully maintaining their tenuous social space, African-American women owned large amounts of property before the war, at the very time when white women were discouraged from such ventures. The book is crammed with facts, and at times the recitation of people and the dollar amount of their property wears thin. Still, Schweninger reveals some tantalizing details. For example, in the lower South some of the wealthiest free blacks took up residence on islands, by 1860 the literacy rate of the wealthiest free black families was higher than that for southern whites, and a sizeable number of property owners had white wives.

The Civil War ruined many wealthy black property

owners of the lower South. On the one hand, some of the richest Creoles of color lost their land or became despondent; the new generation could not cope with the new order. Nor did women play so large a part in ownership after the war. On the other hand, the upper South became more open for property accumulation, especially in cities. In Virginia property holding became a way of life; by 1910 the state's black population owned over 1.5 million acres of farmland. The acquisitiveness of recently freed slaves is startling testimony of a struggle to overcome custom, laws, and sometimes violence. The new generation of property owners came not from the free blacks of the antebellum era but from former slaves who lived in towns or cities and worked as carpenters, masons, undertakers, barbers, druggists, insurance agents, restaurant owners, grocers, and a host of other occupations.

Affluence created tension between blacks and whites, and while Schweninger discusses some conflicts, he does not deal with whitecapping as an organized response to black property ownership. After painting a picture of progress in owning property, the author concludes with the sobering story of Anthony Crawford, a South Carolina black property owner who was killed in 1916 after arguing with a white store owner. Property ownership, after all, offered no security. Schweninger's evidence challenges earlier scholarship that omitted the large number of black property owners. It suggests a number of tangential studies that scholars of southern history should be eager to explore.

PETE DANIEL
National Museum of American History,
Washington, D.C.

JOHN WILLIAM GRAVES. *Town and Country: Race Relations in an Urban-Rural Context; Arkansas, 1865–1905*. Fayetteville: University of Arkansas Press. 1990. Pp. xiv, 332.

This gracefully written, handsomely produced volume is somewhat a disappointment. It seeks to analyze race relations in one state between the end of the Civil War and the dawn of the twentieth century. As with some other studies that have examined this difficult subject, however, the focus is primarily on politics. John William Graves examines struggles over civil rights at the constitutional convention of 1868, clashes between Republicans and Democrats during Reconstruction, the emergence of fusion politics in the 1880s (Democrats sharing offices with black Republicans in predominantly black sections), the growth of the Agricultural Wheel (an interracial agrarian protest organization), and, as the author titles it, the "Journey Backward" during the 1890s and early 1900s as hostile whites pushed through an exclusionary election law, "separate but equal" railroad coach law, a poll tax, and white primaries. This is a familiar story, and although the study does place

these events in a state context, the secondary literature even for Arkansas (including a number of articles by Graves himself) is fairly large. Moreover, there are occasional problems with balance, including three- and four-page sections devoted to a single speech or an individual family.

This is not to minimize the book's several strengths: the remarkable collection of rare photographs (including the grandstand of black entrepreneur Wiley Jones's race track near Pine Bluff), the author's exceptional facility with phrase and sentence (ending the chapter on "The Separate-Coach Law," for example, he writes, "For Arkansas Negroes, the mournful cries of the train whistles in 1891 were both reminders of times gone before and omens of things to come" [p. 163]), the revealing glimpses of the relations between prosperous urban black professionals and business people and various groups of whites, and the evidence that in towns and cities prior to the 1890s there was probably more interracial mixing than de facto segregation. In addition, the volume is successful in comparing the more hardened, anti-black attitudes among whites in rural districts with the more moderate, sometimes genuinely sympathetic attitudes in Little Rock, Pine Bluff, and other towns. Yet the book most often emphasizes well-known themes and only occasionally points to a possible new conceptual framework to unravel the complex, evolving, and often obscured nature of race relations in the South.

LOREN SCHWENINGER
*University of North Carolina,
Greensboro*

MAXINE D. JONES and JOE M. RICHARDSON. *Talladega College: The First Century*. Tuscaloosa: University of Alabama Press. 1990. Pp. xi, 340. \$32.95.

This book gives the story of about one hundred years of the history of an American Missionary Association (AMA) school, Talladega College, founded in east central Alabama in 1867. The study starts at the beginning and then marches slowly but inexorably forward to the century mark, and then on a bit further into the 1970s. Maxine D. Jones and Joe M. Richardson take the reader into every crevasse and corner of the institution's history. Along the way they find a number of interesting men and women, black and white, who shape, and are themselves transformed by, Talladega College. The nineteenth-century white president Henry S. DeForest and the twentieth-century black administrators Arthur D. Gray and Herman Long confronted the problems of educating blacks in the South with determination and imagination. Their experience is worth knowing. The student revolt at Talladega in the 1960s provides a startling example of anger and disruption—a kind of Columbia 1968 in dark minuscule.

But this study lacks the imagination to make the

history of a small (fewer than four hundred students for most of its time) black college an important work. The narrowest kind of institutional history, the book misses the contextual comparisons that might have made it more broadly relevant. Nowhere is Talladega held up against the other AMA creations; Tougaloo and Hampton do not even appear in the book's index. Tuskegee Institute and its omniscient principal Booker T. Washington resided just down the road, but their influence on Talladega is portrayed as tangential. Washington's impact on black education elsewhere in Alabama was decidedly larger, even when it was an opposing force, which was not the case in Talladega.

What we get instead of broad context is institutional detail presented in a too rigorously chronological way. As in so many institutional histories, the book adopts a presidential synthesis for its organization, which leads naturally to a tale told top to bottom. Only rarely do we get much below the head. Talladega College has produced some remarkable graduates in the twentieth century, and to our benefit the profiles of a few do appear in the book. But a systematic overall analysis of who Talladega's students were and what happened to them is missing. What did Talladega do for its students? In the case of several lawyers, educators, and businessmen now prominent in Alabama, it probably did much, although we cannot know that from this book. Admittedly that is a difficult question for a historian to answer, but responses to that kind of question would have made this work far more valuable.

ROBERT J. NORRELL
*University of Alabama,
Tuscaloosa*

ROBERTA SENECHAL. *The Sociogenesis of a Race Riot: Springfield, Illinois, in 1908*. (Blacks in the New World.) Urbana: University of Illinois Press. 1990. Pp. xiv, 231. \$29.95.

Roberta Senechal has written a thoughtful revision of an important upheaval. She describes the murder and an alleged rape that set off the violence in Springfield, Illinois. Once white crowds realized that those accused of killing Clergy Ballard and violating Mabel Hallam were spirited to safety by local police and a prominent businessman, they shifted from would-be lynchers to vengeful rioters who targeted the establishments of elite blacks and whites. Before being quelled by 3,500 state militia, the rioters left two blacks and four whites dead, over 100 injured, and \$120,000 worth of property damaged. They also raised the specter of race war elsewhere and convinced progressives to organize the National Association for the Advancement of Colored People.

Beyond these familiar facts, Senechal provides the first profile of rioters in an interracial outburst that occurred from the turn of the twentieth century to

the Great Migration: single, unskilled white males in their mid-twenties who knew one another. Illinois-born and bred, their wrath emanated from perceptions of black advancement rather than social strain wrought by southern influences, black in-migration, housing competition, or labor disputes that sparked several disorders during and after World War I. As homeowners and boarders living with parents, they drew initial support from influential whites who envisioned the bloodshed as a reformist act (blacks symbolizing the municipal corruption and vice that crossed racial lines and needed cleansing); as working-class residents, they soon frightened these elite allies who opposed the annihilation of black employees and customers. Senechal shows more than class tensions and diverse expressions of racism among whites Springfieldians. She divides the rioters into indictees, arrestees, and injured, with the first, smallest group, containing so-called riffraff and the last comprising those most representative of the mob.

Ironically, Senechal says little about what, if not southerners, volatile incidents, or social strain, sparked the violence in Springfield. What relationship existed between the murder of Ballard and the alleged rape of Hallam, the precipitating events that occurred six weeks apart? What altered white perceptions of black society during that period? What fears and traits did the riot participants—who originally gathered to lynch the assailants—share with lynchers in the South?

These issues aside, Senechal adds considerably to the study of race riots and particularly riot participants. Drawing from newspaper accounts and court and public records, she shows the class, ethnic, and residential diversity of Springfield's rioters and their varied commitments to white supremacy. She applies social science theories to the behavior and, by inference, motivation of participants. She presents critical information that permits the comparison of the Springfield riot and its white male rioters with the interracial upheavals and white participants that clustered around the two world wars. She provides a model that some might find too reliant on journalists for elite opinion and too assured that most of the injured were rioters. Nevertheless, innovative and thought-provoking, Senechal's book must be read by all serious scholars of racial violence.

DOMINIC J. CAPECI, JR.
Southwest Missouri State University

MARTIN BAUML DUBERMAN. *Paul Robeson*. New York: Alfred A. Knopf. 1988. Pp. xiii. 804. \$24.95.

Martin Bauml Duberman's biography of Paul Robeson will excite many scholars and disappoint others. The first historian to be granted access to the Robeson archives, Duberman has approached the task of reconstructing Robeson's life with exemplary thoroughness, scouring the written record, using Free-

dom of Information Act materials, and conducting over one hundred thirty personal interviews. Every aspect of Robeson's remarkable life, from his athletic career to his accomplishments in film, theater, and the concert stage, to his political activism and cold war martyrdom, is covered with meticulous care and scrupulous documentation. Any scholar seeking to understand the trauma of race in twentieth-century America or the power of anticommunism in postwar political life will have to consult this book. Duberman has taken one of the most enigmatic and neglected figures in American political and cultural history and projected him into the mainstream of our historical imagination.

But despite its prodigious scholarship, Duberman's book falls short of being the literary tour de force that many hoped it would be. Robeson's life is one of the great American tragedies, the story of an African American whose talents were too large and personality too compelling for the society he was born in. Continually assailed by minor and major indignities, he transformed his prominence as an artist into a platform for political protest and seized on the Soviet Union—where he experienced no color prejudice—as a battering ram against Western racism and imperialism. Robeson's combination of racial egalitarianism and militant pro-Sovietism made him the target of a campaign of harassment that lasted fifteen years and destroyed his career, his health, and ultimately his sanity. Every government agency concerned with prosecuting the cold war—the State Department, the Federal Bureau of Investigation, the Central Intelligence Agency, congressional investigating committees—made Robeson a target, and every important black organization felt compelled to denounce him. This is the Robeson story that readers need to ponder, but Duberman fails to make it the central theme in his drama. Instead, Duberman gives us an excruciatingly detailed picture of everything that Robeson did in public and private life from the time he was born to the time he died. Amid endless accounts of love affairs, family feuds, and concert performances, some of the dignity and larger meaning of Robeson's life is lost. Duberman does cover all of the important bases—Robeson's growing rage at American racism, his pioneering views on the multicultural roots of American civilization, his imprisonment in a Stalinist world view that ultimately isolated him from his own people—but he smothers these insights in excessively detailed accounts of Robeson's private life.

Duberman's thoroughness makes his biography an invaluable source for other historians, but those seeking an assessment of Robeson's importance in American civilization will have to turn elsewhere. Duberman takes the historian's virtues—caution and circumspection—to a painful extreme. The book features hundreds of quotations from contemporaries about Robeson's personality and motivation but makes no attempt to synthesize them into a coherent

portrait. Duberman missed the opportunity to create a political parable of compelling force—something that illuminates American race relations the way Isaac Deutscher's *Stalin* illuminated the Russian revolution. In ways that Duberman fails to emphasize, Robeson was a prototypical African American, someone whose pursuit of the American dream of achievement and success brought bitterness and humiliation. The major themes in his life still resonate with undiluted force.

Properly received, Duberman's biography represents the opening of a political debate about one of twentieth-century America's most intriguing and important figures. We are all in Duberman's debt for the care and intelligence with which he approached his task.

MARK NAISON
Fordham University

WILLIAM D. JENKINS. *Steel Valley Klan: The Ku Klux Klan in Ohio's Mahoning Valley*. Kent, Ohio: Kent State University Press. 1990. Pp. xii, 222. \$27.50.

As viewed in the work of such scholars as Robert K. Murray, Kenneth T. Jackson, David Chalmers, and John Higham, the Ku Klux Klan of the 1920s was a product of frictions stemming from nationalism triggered by World War I, the Red Scare, and the labor struggles of 1919. Jackson's work has been particularly influential in elaborating the thesis that the Klan of this period, predominantly an urban phenomenon, was in the main a lower-middle-class movement composed of blue-collar nonunion employees of large business enterprises. Klan members were impelled by their resentment of recently arrived immigrants. In this book, William D. Jenkins presents a case study that moves away from the established synthesis of treating the Klan as a response to national trends and issues. Jenkins focuses on the local factors in the situation and on the cultural conflict between the new immigrants and pietistic Protestants concerned with safeguarding traditional Anglo-Saxon folkways. We are given a well-researched, clearly written account of the Klan as it functioned in Ohio's Mahoning Valley, although the interpretive framework is not wholly satisfactory.

The point is made that Klan leaders opted for a flexible recruitment strategy that tailored national policies to local issues and so built an organization that included both moderates and extremists. By the early 1920s, conditions in Youngstown and its surrounding communities provided fertile ground for the growth of the secret order. In 1920 two-thirds of Youngstown's population was foreign-born or the children of immigrants, and native pietistic Christians, according to Jenkins, resented the cultural style of newcomers who came increasingly from eastern and southern Europe. The Klan emerged as an expression of Protestant moral reform that identified

the immigrants with various forms of corruption and lawlessness.

In 1923 the Klan succeeded in electing Charles Scheible as mayor of Youngstown. With a background in business, Scheible, though colorless, was able to project an image of honesty and efficiency. For those drawn to flamboyant politics there were the supporting roles of demagogues Clyde Osborne and Col. Evan A. Watkins. Jenkins provides an interesting account of the election campaign, stressing the Klan's ability to capitalize on law and order concerns and the disarray of those opposed to the organization. Also useful is the information given about the Klan victories in the nearby communities of Warren, Niles, Girard, and Struthers. Additionally, Jenkins recounts the dramatic story of the Niles Riot of 1924 in which immigrants, especially Italian and Irish immigrants who organized the Knights of the Flaming Circle, effectively fought the Klan.

The work is most convincing in the evidence outlined as to the base of public support on which the Klan rested. The majority of Klan members were clearly not marginal in their socioeconomic status. With an expensive dues structure, the Klan did not particularly attract those from the lower classes; the broadest appeal was to the middle class. Recruitment patterns and the attempt to develop a Klan country club all point to a middle-class orientation. At least as far as Youngstown is concerned, Jackson's thesis about the social composition of the Klan is overstated. The book is also persuasive in rejecting any notion of linkage between religious fundamentalism and Klanism.

There are, however, some problems with this book. It is striking that little attention is paid to the controlling presence of the steel industry in the valley. An official of Youngstown Sheet and Tube is quoted as believing in immigrant progress toward citizenship although that comment accompanies his observation that these immigrants "had never mounted to spiritual heights" and that they were "more than ordinarily susceptible to unsound social and political propaganda" (p. 22). But Jenkins fails to see that the company housing furnished by Sheet and Tube, divided into American, foreign-born, and black sections, fostered the ethnic and racial divisiveness exploited by the Klan. The study would have benefited from further exploration of corporation policies in the area as it would also have been strengthened by greater attention to the relationship between local events and national trends.

Jenkins's work, added to studies by Charles C. Alexander (*The Ku Klux Klan in the Southwest* [1966]) and Robert A. Goldberg (*Hooded Empire: The Ku Klux Klan in Colorado* [1981]), broadens our perspective on the Klan and helps us understand why an industrial, northern state such as Ohio became a major center of the order. The work, however, of integrating this local history into a broader analysis of the politics of

fear and bigotry the Klan represented is still to be done.

HERBERT SHAPIRO
University of Cincinnati

LYNN D. GORDON. *Gender and Higher Education in the Progressive Era*. New Haven: Yale University Press. 1990. Pp. xiii, 258. \$29.95.

Beginning her reflections on college women, higher education, and social activism as an undergraduate at Barnard in the turbulent 1960s, Lynn D. Gordon here examines the intersections of educated women, their institutions, and American reform in the complex, transitional years 1890–1920. She explores the experience, community, and divergence of two generations of women at five highly varied institutions: the University of California at Berkeley, the University of Chicago, Vassar College, Agnes Scott College, and Sophie Newcomb College. (Refreshingly, there are no New England case studies.)

Generational divisions are always somewhat problematic. Gordon's two generations include the traditional pioneers, the pre-1890 graduates, and an elongated second generation born after 1880 and distinguished by the "linking of gender consciousness to campus life and to post graduate plans for social activism, a growing commitment to egalitarian rather than separatist feminism, and a simultaneous interest in marriage" (p. 5). Essentially, her pioneers are faculty and deans; her second generation encompasses the students, united by their continuity of experience in the Progressive era.

Gordon details the successful strategies of separate social and equal curricular and intellectual spheres promoted by the pioneer generation at coeducational Berkeley and Chicago for their successors. First-generation leaders such as Lucy Sprague and Jessica Peixotto at Berkeley and Marion Talbot at Chicago worked with women philanthropists for dormitories and connections for their students. When the University of Chicago, worried about effeminization, moved to establish a gender-segregated curriculum, off-campus networks unleashed a national barrage in opposition. This campus linkage with women's organizations and reform causes forged by the pioneer generation was even more visible at Vassar.

Whereas Gordon establishes that the most important determinant of the undergraduate experience, at least for the first generation of college-educated women, was the choice between a single sex or coeducational institution, she also demonstrates the importance of region and the long shadow of the southern lady in the founding and later development of Agnes Scott College and Sophie Newcomb College. Although generational distinctions used by Gordon are least effective here, the second generation, as at Berkeley, Chicago, and Vassar, found opportunities

for service and reform that gradually eased the gender separatism on campus and beyond.

Gordon ably shows, at each of the institutions, the early importance of community and women's culture and the hopes of the pioneers for lives of broadened potential for their talents. Creatively employing student writings, she demonstrates the two generations moving apart. Short stories in the *Vassar Miscellany* featured talented women losing their men; indeed, at all of the campuses "usually the female protagonist lost the man or job" (p. 39). During the 1910s, Gordon argues, the students and recent graduates stressed equality more than separation, showing a greater emphasis on heterosociality and finally marriage.

The strength of Gordon's study is in the dialogue she is able to frame between the generations, using the correspondence and biographical studies of pioneers and tracing the changing aspirations of the second generation in their letters and articles in newspapers and literary and alumnae magazines. Gordon's study is richly peopled and fully alive; it is a fine addition to the burgeoning field of the history of women in higher education.

DOROTHY M. BROWN
Georgetown University

NANCY F. GABIN. *Feminism in the Labor Movement: Women and the United Auto Workers, 1935–1975*. Ithaca: Cornell University Press. 1990. Pp. xi, 257. Cloth \$31.95, paper \$12.95.

In the historiography of postwar America, and even more so in textbooks and classrooms, the labor movement and the women's movement have been treated as separate stories. The builders of industrial unionism are assumed to be men, while the feminist struggle is explained as the product of middle-class women's rejection of suburban housewifery or oppression by male civil rights workers and antiwar activists. Nancy F. Gabin's study makes it impossible to continue to tell two separate stories. She convincingly shows that working women played a crucial role in industrial unionism and demonstrated a long-term commitment to feminist ideals that predated, and differed from, the more middle-class feminism of the 1960s and 1970s.

Gabin organizes her book chronologically. She begins in the 1930s, arguing that although the United Auto Workers (UAW) succeeded in recruiting women workers in auto plants by appealing to their class interests, the union shared management's commitment to gender hierarchy in the workplace. Imbued with a traditional idealization of the family wage earned by the male head of the household, unionists—including many women—did little to challenge sex discrimination in job classifications, pay scales, seniority lists, and union leadership. Then World War II brought thousands more women into automo-

bile and aircraft plants, and into jobs traditionally considered male. When female workers demanded equal pay and seniority status as well as provisions for maternity leave and child care, they confronted a UAW allied with employers to limit women's access to permanent postwar jobs. In time, the UAW came to recognize that its own survival depended on enlisting these new auto workers in the union cause and, in 1944, it established a Women's Department to respond to female concerns. But the reconversion of factories to peacetime production only intensified gender conflict in the UAW. As women workers struggled to keep well-paying jobs and hard-earned seniority, they clashed with male workers who preferred that they return home or to lower-paying "female jobs." Although women's protest at the end of the war did not succeed in keeping them where they wanted to be on the shop floor, it did get them more access to the union hall. There they made the case that gender equality was inseparably tied to the larger equality promised by industrial unionism.

During the postwar era, UAW women continued to push for equal rather than differential treatment, culminating in their drive during the mid-1960s to repeal protective legislation at the state level, which employers were using to defend job discrimination against women. In this way and in their use of Title VII of the Civil Rights Act of 1964, working-class women anticipated positions that middle-class feminists would popularize later. UAW women also continued their struggle for power within the union, finally winning in 1966 a seat on the union's International Executive Board. Gabin concludes that women in the UAW developed a unique kind of working-class feminism more concerned, for example, with equality at the workplace than in the home and the larger society.

Gabin's book is important not just for what it says about class difference within feminism, but also for what it reveals about the way unions have been shaped by women committed to making them responsive to female needs. My only complaint is that, despite her careful research and cogent analysis, Gabin's lack of sustained attention to the motives and actions of particular women, both leaders and rank and file, reinforced by her tendency to make policies and other abstractions the subjects of sentences, will make her book less accessible to the general reader than it might have been. Nonetheless, specialists will find this work a new guide to feminism and unionism in the postwar era, and hopefully in time Gabin's findings will filter down from specialists to generalists.

LIZABETH COHEN
Carnegie Mellon University

ROGER M. OLIEN and DIANA DAVIDS OLIEN, *Easy Money: Oil Promoters and Investors in the Jazz Age*. Chapel Hill:

University of North Carolina Press. 1990. Pp. xi, 216. Cloth \$29.95, paper \$11.95.

Historians are already beginning to offer up their postmortems on the meaning of the 1980s, and their conclusions sound hauntingly like the traditional descriptions of the 1920s: the growing disparities between the rich and the poor, the large-scale accumulation of debt, the severe weaknesses in the money markets, the suffering of small farmers, and the illusion of prosperity camouflaging serious structural weaknesses in the economy. Traditionally, the images of speculation and materialism in the 1920s revolved around the stock market boom and the Florida real estate bubble, but new research continues to show that greed and economic recklessness were far more pervasive. Roger M. Olien and Diana Davids Olien expose another sector of the American economy in the 1920s, and here, too, the get-rich-quick, easy-money mentality dominated business thought and investor expectations.

The Oliens have done their homework here. This study is an outstanding piece of scholarship that is well researched, intelligently conceived, and briskly written. The authors have marshaled an astonishing variety of primary sources: federal, state, and local court records; newspapers from major metropolitan areas as well as obscure small towns in Texas, Oklahoma, Louisiana, New Mexico, and California; federal, state, and local land records; manuscript collections from the federal government, state libraries, corporate archives, and private holdings; trade journals and trade association files; and business and financial journals. The book is full of slick con men, sleazy promoters, and gullible investors, just what one would expect to find in the greedy atmosphere of popular culture and private capitalism in the 1920s. Fortunes were made and fortunes were lost, and the financial well-being of hundreds of thousands of Americans was ruined in the oil mania of the decade.

In addition to exposing the dark side of the culture of capitalism in the 1920s, the book also provides an engaging portrait of the oil industry during the decade. Like the securities and real estate markets, the oil business went through a boom and bust cycle in the 1920s. The emergence of the automobile as a mass consumer item created an unprecedented demand for petroleum products, and the opportunities for wealth and the demand for investment capital were equally unprecedented. Legitimate wildcatters and illegitimate grifters fanned out across the southwestern landscape hunting for green money and black gold. They found both. But a steady series of big discoveries in California, Texas, and Oklahoma dramatically increased oil supplies by mid-decade and depressed oil prices. Declining prices set in motion another example of the "poverty of abundance" dimension of the Great Depression. To deal with falling prices, oilmen pumped more oil, which only drove prices down even more.

The boom came to a dramatic bust in 1930 when the huge oil fields in east Texas were discovered. The American economy was swamped in a flood of oil; prices collapsed, thousands of producers went bankrupt, and hundreds of thousands of investors lost billions. The industry did not really recover for another forty years. The Oliens have made an important contribution to American business and social history. More so than was thought before, the decade of the 1920s was a time of excess and poor economic judgment, and the entire country eventually had to pay the debt in the 1930s. One can only shudder about what historians will someday say about our own time.

JAMES S. OLSON
Sam Houston State University

WILLIAM S. WORLEY. *J. C. Nichols and the Shaping of Kansas City: Innovation in Planned Residential Communities*. Columbia: University of Missouri Press. 1990. Pp. xxv, 324. \$29.50.

J. C. Nichols is best known as the developer of Country Club Plaza (1922), the earliest automobile-oriented regional shopping center and thus the prototype for the postwar American shopping mall. As William S. Worley shows in this thoughtful, well-written, and well-researched book, the Country Club Plaza was only one element in Nichols's lifelong effort to create for Kansas City's upper middle class an ideal suburban refuge, a protected environment of privilege that he called "The Country Club District."

From 1908 until his death in 1949, Nichols worked to adapt the forms of the late-nineteenth-century railroad and streetcar suburb to the needs and the social structure of the automobile age. Specializing in "high class" suburbs through the 1920s, Nichols then pioneered in the creation of mass-produced, mass-market-oriented developments as early as 1940. A highly influential leader in his profession, Nichols helped to shape not only Kansas City but also the nation.

Yet, as Worley shows, Nichols was no visionary. He was instead a synthesizer, a careful student of others' work from which he was constantly borrowing the most useful elements. He was relentless in his careful planning and comprehensive restrictions to protect his district. He sought to create a quiet country village for affluent urban businessmen: a picturesque home-centered environment that would permanently retain high land values. From the 1920s through the 1980s the Country Club District has remained the center of affluent Kansas City.

Like Nichols, Worley does not seek to distinguish himself through radical innovations. For interpretation he adapts and synthesizes the best of recent suburban history. If he lacks Michael Ebner's sense of locality or John Stilgoe's understanding of architectural history or Mary Corbin Sies's insights into "the

professional-managerial-class," Worley nevertheless excels in his understanding of suburban development as a business. He is as careful as Nichols in ascertaining profits and losses and tracing the complex transactions that are the real substance of land development. He thus provides our best in-depth portrait of a major suburban developer operating through the booms and busts of the first half of the twentieth century.

This businesslike approach fits Nichols and his enterprises well, except, ironically, when Worley examines Nichols's showpiece, the Country Club Plaza. Here Nichols really did create a new form, one whose originality and implications went far beyond Nichols's initial goals to preempt ugly strip development and profit from the retail revenue generated by his residential developments. Almost from the beginning, the Country Club Plaza was designed to rival Kansas City's downtown as the site for affluent consumption. It succeeded so well that, as Worley points out, by the 1980s the Plaza had become "the new downtown of Kansas City" (p. 263).

The Plaza thus had radical implications for urban form. To understand its origins one must look much further into the 1920s "car culture" than Worley has done; and to understand the Plaza's impact one must look further into regional theory. Regardless of his intent, Nichols's developments have led ultimately to a radical decentralization of urban space that, in Kansas City as elsewhere, has replaced Main Street with megalopolis. This is an odd legacy for an archetypal midwestern booster-businessman whose aesthetic ideal was the country village and whose principal value was permanence.

ROBERT FISHMAN
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Camden

ROBERT F. BURK. *The Corporate State and the Broker State: The Du Ponts and American National Politics, 1925-1940*. Cambridge: Harvard University Press. 1990. Pp. xi, 359. \$35.00.

Robert F. Burk's book is a welcome and useful study of the political activities of the du Pont family from the mid-1920s through the New Deal. Based on meticulous research in relevant collections at the Hagley Library and elsewhere, the book focuses particularly on the three du Pont brothers (Pierre, Irénée, and Lamont) and their close business associate John J. Raskob, detailing with admirable clarity and precision the role that one segment of "big business" actually played in public affairs.

Burk tells the story in fifteen well-crafted chapters, two of which provide background and the last of which serves as an epilogue. His story proper begins with four chapters on the work for prohibition repeal and its political ramifications, proceeds through two chapters on the du Ponts and the early New Deal, and

continues with another six chapters on the American Liberty League. Along the way it covers much familiar ground. But it also provides much new and often rich detail, breaks new ground in delineating the extent and forms of du Pont involvement, and argues persuasively that their organizations were intended as demonstration projects in elite governance.

The book's most innovative aspect is Burk's argument that the du Ponts were corporate statistes rather than Jeffersonian democrats or American Tories. In the 1920s, they pioneered a form of welfare capitalism, and, as they saw it, what America really needed was a return to the hierarchical ideals of the Founding Fathers through the stewardship of a corporate gentry combining economic with political power and governing through a supercouncil resembling the War Industries Board of World War I. This was the meritocracy best equipped to deal with modernity. And in the early New Deal, the du Ponts saw movement toward their ideal and were generally supportive. The break came when the New Deal began evolving into a "broker state" that "legitimized the rival claims of competing interests and maintained dual structures of public and private governance" (p. ix); and it was against this that the American Liberty League fought and lost, the result being the eclipse of "corporate conservatism" by an accommodating and more sophisticated "corporate liberalism."

Much of this is persuasive. But Burk is better at political detailing than in reconstructing the clash of ideological constructs. We learn that the du Pont brand of corporatism differed from the associational brand of Herbert Hoover and the tripartite brand of Gerard Swope. Yet Burk says little about the larger intellectual current of which these men were a part and nothing about the "center" and "left" varieties featured in the works of Steve Fraser and Donald Brand. Nor is there much on the corporate pluralist formulation of a "concert of interests" that competed with broker-state pluralism in New Deal thinking and action. As one reviewer has already noted, the book's subtitle more accurately conveys its content than does its title.

Still, this is a remarkably good book, deserving high marks for its readability and its incisive and authoritative treatment of an important subject. It merits wide reading both by scholars and others.

ELLIS W. HAWLEY
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JOHN C. MCWILLIAMS. *The Protectors: Harry J. Anslinger and the Federal Bureau of Narcotics, 1930–1962*. Newark: University of Delaware Press. 1990. Pp. 251. \$37.50.

Harry J. Anslinger (1892–1975) was commissioner of the Federal Bureau of Narcotics (FBN) from 1930 (when it was established) until his retirement in 1962. The FBN was always small (averaging 242 agents

under Anslinger), but its commissioner's influence was great: for thirty-two years, Anslinger had the closing argument in the debate whether the police or the doctors should handle the nation's narcotics problem.

John C. McWilliams has shown how Anslinger's unswerving commitment to the crime control model of drug policy endeared him to congressional patrons who installed him in an office that he (and they) claimed made him the nation's leading authority on drugs; they then used that (bogus) authority to legitimate policies so patently ineffective as to seem lunatic to anyone except a congressman or a district attorney who wanted to be one. Anslinger's congressional appearances were droll affairs where the representatives and the drug czar would chuckle genially at naive public health medicos foolish enough to suggest that there was anything wrong with the doctrines Anslinger and Congress promoted. Based on the Anslinger papers at Penn State's Pattee Library, supplemented by interviews with officials and agents of the FBN and colleagues of Anslinger, McWilliams's book is a well-informed guide to the history of U.S. narcotics policy from the Harrison Narcotic Act (1914) until the end of the FBN's autonomy when it was merged into the Bureau of Narcotics and Dangerous Drugs in 1968.

McWilliams shows skill and perseverance in his detailed analysis of the bureaucratic talent that allowed Anslinger to survive and flourish. But Anslinger was a busybody who could never resist the opportunity to gate-crash a public policy party, that is, if there might be any publicity door prizes. McWilliams cannot resist following Anslinger down these byways, sometimes, I am afraid, because it gives him the chance to write about topics, such as organized crime or foreign intelligence, more interesting than narcotics policy.

One of these digressions got McWilliams into a row when a piece of this book appeared in the *Pennsylvania Magazine of History and Biography* (April 1989). In his memoirs Anslinger claimed that he secured a source of drugs for a morphine-addicted "congressman" so the lawmaker would not have to deal with criminals. In *Pennsylvania Magazine* (p. 222), McWilliams said flatly, "The man was Senator Joseph McCarthy," based on a 1978 magazine article by Maxine Cheshire and an interview with an FBN agent. A reporter from the *Philadelphia Inquirer*, lured to McWilliams's article by the McCarthy nugget, smelled scandal when David Oshinsky and Thomas Reeves, the two standard McCarthy biographers, both rejected the story and the FBN agent admitted that he himself had gotten the idea from Cheshire's story (*Philadelphia Inquirer*, April 3, 1989, p. A-9). *Pennsylvania Magazine* (p. 512) carried an erratum in its July 1989 issue that the McCarthy identification "should read 'The evidence strongly suggests that the man was Senator Joseph McCarthy.'" In the book McWilliams (grudgingly?) gives a bit more ground,

writing, "Evidence suggests the mystery politician was Senator Joseph R. McCarthy" (p. 99). From "was" to "strongly suggests" to "suggests": frankly, the evidence is so weak that the McCarthy identification should have been removed from the work. Even making allowance for McWilliams's belief that he is right, and conceding that the *Philadelphia Inquirer* lost all sense of proportion reporting the story, McWilliams should at least have included a note in the book setting forth the dispute, particularly because his citation of Oshinsky and Reeves on a related matter (the cause of McCarthy's death, p. 99) creates the false impression that they agree with his conclusion. McWilliams's refusal here to abandon a theory rejected by the informed historical community shows a stubbornness worthy of a better cause. This is a foolish and maddening blemish in an otherwise sensible and well-balanced examination of a significant figure in U.S. political history.

RICHARD GID POWERS
College of Staten Island,
City University of New York

ROGER J. SANDILANDS. *The Life and Political Economy of Lauchlin Currie: New Dealer, Presidential Adviser, and Development Economist*. Durham, N.C.: Duke University Press. 1990. Pp. xi, 441. \$57.50.

Roger J. Sandilands, a former student and associate of Lauchlin Currie, has written an engaging analysis of this intrepid practitioner's life and thought. Having full access to Currie and his papers, Sandilands was commissioned to produce the book by the Colombian Savings and Housing Institute. A trade association, the institute promotes interests of private housing finance corporations, which were a principal means to implement Colombian policies designed by Currie to turn construction and urbanization into engines of self-sustaining economic growth. Sandilands's work merits considerate scrutiny as the first full-length examination of a pragmatic liberal, theorist, adviser, and planner in behalf of capitalist development.

Born in Nova Scotia, Currie completed undergraduate studies at the London School of Economics and earned his Ph.D. under Allyn A. Young at Harvard in 1931. A member of the New Deal's brains trust that assembled in 1934 in the Treasury Department, Currie was from 1939 to 1945 the first professional economist, according to Sandilands, who was attached to the White House. There he dispensed advice on employment, social security, and military preparations, among other things. He also traveled on assignment to China. By this time Currie knew many influential American economists and policy makers. Aware of an adviser's limited power, he experienced the vicissitudes of politics and was tarred unfairly by McCarthy-era allegations that he was a communist sympathizer.

In the next phase of his professional life, Currie headed the World Bank's mission to Colombia in 1949, the first comprehensive effort by an international agency to investigate a country and make recommendations on development policy. Subsequently advising on Colombian policy implementation with the U.S. Public Administration Service, the Point Four Program, the U.N. Technical Assistance Program, and Colombia's Economic Development Committee, Currie learned more about political difficulties that block or distort the authoritative reforms that he wanted combined with national planning mechanisms and a coherent, empirically based vision.

Currie's experience led inexorably toward elaboration in 1961 of his integrated plan for Colombian development, President Misael Pastrana Borrero's implementation in 1971 of the scheme's main elements, and elaboration of a theory of economic growth constructed on the prior formulations of Young. For the Colombian strategy of 1971, whose elements remain in place, Currie mobilized construction and exports as leading sectors to exploit underused resources and draw labor from an increasingly efficient agriculture into the higher-wage occupations of an urbanizing economy. The system's operation required creation of private finance institutions and, to avoid inflation and promote growth, sound government planning and management to stimulate and direct savings and capital formation. Sandilands believes that the evidence vindicates Currie's framework. He contends that Currie's subsequent writings "undermine the marginal productivity explanation of growth and income distribution and suggest radically different policies to promote faster development and better distribution" (p. 368).

Awkward mixtures of narrative, economic analysis, and sometimes inadequate historical description occasionally cause confusion. Sandilands lists Currie's publications from 1931 to 1990, which will surely facilitate further evaluation by historians and development theorists. This review relates some highlights of Currie's thought. Generally hopeful about modern capitalist development, Currie also contributed to the discourse on topics such as central banking, resource degradation and conservation, demographic relations of economic growth, and the design of what he called "cities-within-cities" (p. 265).

ROBERT N. SEIDEL
State University College of New York,
Empire State College

WILLIAM B. PICKETT. *Homer E. Capehart: A Senator's Life, 1897-1979*. Indianapolis: Indiana Historical Society. 1990. Pp. xv, 243. \$27.95.

The media tended to depict Senator Homer E. Capehart as a rigid right-wing ideologue. William B. Pickett's biography refutes this as an erroneous caricature by revealing that Capehart was far more flexible than

has heretofore been appreciated. He was, rather, "the archetypical midwestern Republican" (p. ix) and a pragmatist.

Born in rural Indiana in 1897 and achieving only a high school education, Capehart became a highly successful salesman of Butter-Kist popcorn machines and a multi-million dollar entrepreneur with the Capehart Automatic Phonograph Corporation. He influenced the nation's listening habits with the record player and jukebox. Capitalism was a system that he knew firsthand.

Having accumulated wealth and possessing a fine speaking voice and outgoing personality, Capehart gravitated to Republican politics. Positioning himself as a "Republican Roosevelt," he won the senate seat from Indiana in the election of 1944. He held it for eighteen years.

Whereas Capehart advocated free enterprise, he accepted many New Deal measures as necessary and proper contributions to the social well-being of the nation. He was cautious about unbalanced budgets and thus advocated a pay-as-you-go approach in the Fair Deal–New Frontier eras. Until the Eisenhower era, Capehart was a staunch provincial isolationist of the Taft wing. He made a major shift toward internationalism because of Ike's persuasiveness.

Capehart was a precursor of the anticommunist theme; when Senator Joseph McCarthy's tactics offended him, however, he subtly moved away from blatant red-baiting. Eventually, he broke with his right-wing Indiana senate colleague, William Jenner, who called him a "New Deal Sonofabitch" (p. 163).

By 1962, running against a youthful Birch Bayh, Capehart had acquired an image of an old-fashioned senator: overweight, bespectacled, cigar-smoking, sparse hairline, and a double chin. Classifying himself as a "progressive conservative" (p. 161), he nevertheless could not appeal to the Yuppie generation and narrowly lost his bid for a fourth term. In retirement, he remained active in both business and politics as a party organizer and fund raiser.

The factual narrative plus the absence of an overly analytical approach (that is, in which the historian displays his or her superiority of judgment over that of the political figure at hand), produces a nonstereotypical portrait of a successful but not faultless politician. He was, in fact, an "archetypical American politician." After all, what can be more American than popcorn, jukeboxes, and a self-made man?

FREDERICK H. SCHAPSMEIER
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JULIAN M. PLEASANTS and AUGUSTUS M. BURNS III.
Frank Porter Graham and the 1950 Senate Race in North Carolina. (The Fred W. Morrison Series in Southern Studies.) Chapel Hill: University of North Carolina Press. 1990. Pp. xv, 356. \$29.95.

Since World War II, a number of progressive-minded Southerners in Congress have been felled by the politics of race—House members Brooks Hays of Arkansas, Carl Elliott of Alabama, and Frank E. Smith of Mississippi, and senators Claude Pepper of Florida and Ralph Yarborough of Texas, to name but a few. Although some (except the resilient Pepper) have gradually faded from public consciousness, the memory of one such casualty, Frank Porter Graham of North Carolina, has assumed the proportions of a minor legend. Graham's persistent name recognition is attributable in part to the fact that he was such an anomaly in southern politics: an academic with outspokenly liberal leanings and a long history of association with organizations then considered left-wing. It may stem as well from a romanticized notion of Graham as martyr: numerous North Carolina admirers described this longtime president of the Consolidated University of North Carolina as "saintly," a description that his Senate colleague Wayne Morse elevated to "Christ-like." Julian M. Pleasants and Augustus M. Burns III, more objective observers than Morse, characterize "Dr. Frank" as innocent, self-confident, ambitious, disorganized, candid, principled, idealistic, and uncompromising to a fault. Some of these personal qualities contributed to the brevity of his tenure in the Senate.

An appointee to the office, Graham served only eighteen months; in terms of legislative achievement, his Senate career amounted to nil. His image persists, then, more as a symbol of failed liberalism than of a seasoned politician felled by "dirty tricks," such as, for example, Alabama's Elliott, coauthor of landmark programs of federal aid to education and libraries.

Based on their in-depth study of North Carolina politics, the authors conclude that Graham snatched defeat from a resounding first primary victory because of in-state factional struggles coupled with his lack of appeal to blue-collar whites traditionally vulnerable to the use of race as a political weapon. In a broader context, the schema employed in this Democratic primary of 1950 strikes a contemporary reader as a precursor of campaigns yet to come, such as the presidential campaign of 1988, the senatorial race in North Carolina of 1990, and the gubernatorial contest in Alabama of 1990. Racial hostilities were deliberately manipulated to divert North Carolina's predominantly white electorate from pocketbook and international issues; Graham's opposition flaunted the image of an individual black male to dramatize the supposed dangers of racial integration. Jesse Helms, an ambitious radio news director in 1950, observed the upset of Graham from his position as a minor functionary in the campaign of Graham's opponent, Willis Smith.

Focusing on an improbable politician but an engaging protagonist, Pleasants and Burns have written a model political study, painstakingly researched and

analyzed, absorbing to read, probably the definitive account of Graham's brief trial by political fire.

VIRGINIA V. HAMILTON
University of Alabama,
Birmingham

NANCY J. WEISS. *Whitney M. Young, Jr., and the Struggle for Civil Rights*. Princeton: Princeton University Press. 1989. Pp. xv, 286. \$24.95.

Whitney M. Young, Jr., writes Nancy J. Weiss, was "a black man who grew up in a middle-class family in the segregated South, [and] spent most of his adult life in a white world, transcending barriers of race, wealth, and social standing to build bridges between the black ghetto and the white establishment" (p. xi). He resided with his parents and siblings at Lincoln Institute, a 444-acre boarding school located in Shelby County, Kentucky, that was established by Berea College after the state legislature barred blacks from attending that institution. His father, Whitney, Sr., rose to become principal of the school, a job that had traditionally been filled by a white man.

Whitney, Sr., concludes Weiss, "honored the rules of the white man's game . . . in a way that preserved his dignity. . . . It was a lesson that his son would later translate into his own terms" (p. 13). Enrolling at Lincoln at the age of thirteen, Young later graduated as valedictorian and pursued college studies at Kentucky State Industrial College in Frankfort. Young's academic performance during his first two years at Kentucky State seemed to indicate that Lincoln had not adequately prepared him for college studies. Yet, the nurturing, encouragement, counseling and self-confidence he received both at Lincoln and Kentucky State compensated for the inferior academic preparation both institutions had to offer. Having successfully completed four years of college, Young saw his plans for graduate or professional school interrupted by World War II.

Not abandoning his dream of entering the medical profession, Young took the Army General Classification Test and scored high enough to be accepted into the Army Specialized Training Program, convinced that this route "would be a means of going to medical school" (p. 31). As fate would have it, he was sent to the Massachusetts Institute of Technology to study electrical engineering. On his arrival, Young and three other blacks decided that they would resist being segregated from the white trainees. Receiving an honorable discharge from the army, Young joined his wife, Margaret, who was a student in the master's degree program in educational psychology at the University of Minnesota. His less than distinguished undergraduate academic record nearly prevented his admission into the graduate program in social work at Minnesota. Young's mentor, John C. Kidneigh, associate director of Minnesota's School of Social Work, proved invaluable to him as he raised probing ques-

tions in the area of race relations: What were the different ways of combatting prejudice and discrimination? Would a law prohibiting discrimination really help? Or did one have to work at changing people's hearts and minds? These questions and others remained foremost in Young's thoughts as he assumed positions of leadership in Urban League locals, as Dean of the Atlanta University School of Social Work, as Executive Director of the National Urban League (NUL), and as he consorted with the nation's economic and political white elites.

Weiss has written a well-documented and historically solid account of Young's formative years and his tenure in the Urban League movement. It is her analyses and interpretations that are open to question. Young was of that generation of college students who perceived themselves not as blacks but as Americans. He did not "honor the white man's game," and refused to accept discrimination and racial segregation as a fact of life, as did his father. The author's argument that Young propelled the Urban League into the vanguard of the civil rights movement is unconvincing. Whereas he may well have "moved his organization toward a more activist stance," Young wanted, for example, assurances from the leadership of the March on Washington in 1963 that this protest activity would have to be "a general expression of our concern about the problem of employment and infringement of . . . civil rights" before committing the league's support of the march (p. 103). At least eight years prior to Young's tenure as executive director of the NUL, local leagues had joined in and supported public demonstrations, something that both Lester Granger, Young's predecessor, and Young found hard to accept. Finally, although Young early on recognized the importance of civil rights in racial advancement, he believed that social work, the league's hallmark, should have been what guided the organization as it fought for wider opportunities and civil rights for African Americans.

Despite the flawed analyses and interpretations, Weiss's book on Young and his role in the civil rights movement is an important contribution. It is well-written, thoroughly researched, and accurate in depicting events. My criticisms are inconsequential compared to the value of this book.

JESSE T. MOORE, JR.
University of Rochester

DENTON L. WATSON. *Lion in the Lobby: Clarence Mitchell, Jr.'s Struggle for the Passage of Civil Rights Laws*. New York: William Morrow. 1990. Pp. 846. \$24.95.

Denton L. Watson performs a useful service by giving us the first biography of Clarence Mitchell, Jr., head of the Washington, D.C., bureau of the NAACP from 1950 to 1978. A vital figure in the mid-century civil rights movement, Mitchell's role deserves to be better known. His values and methods indelibly stamp him

as a member of the old-line black bourgeoisie and, consequently, we are reminded of the significance of class in the African-American community. More important, Watson's well-researched and interview-laden book relates the intimate, behind the scenes particulars of the political machinations involved in passage of the civil rights laws of 1957, 1960, 1964, and 1968.

Despite its virtues, the book would have benefited greatly from more careful editing. It is marred by choppy organization and abrupt transitions that make the overly long narrative difficult to follow. The manuscript also suffers from the inclusion of too much extraneous detail that detracts from its primary focus. Does anyone really need to know, for example, both the French lines and their English translation from the play *Sleeping Beauty*, in which Mitchell appeared in high school?

While Watson provides more information than necessary about some topics, he gives too little about others. The reader does not get a clear picture of the effect on the association of tensions between Mitchell and Roy Wilkins caused by Mitchell's disappointment when Wilkins was appointed over him as secretary of the organization. Whereas he reveals antipathy between the NAACP and newer civil rights groups, Watson, himself director of public relations for the association, seems to believe that to admit any shortcomings of the NAACP would diminish the historical assessment of the organization. Accordingly, he fails to acknowledge the crucial role played by other individuals and groups in gaining greater racial equality. For example, Watson alludes to animosity between Martin Luther King, Jr., and the NAACP, but does not indicate the times they successfully worked together, as in the March on the Conventions Movement of 1960. From this book one comes away thinking that the Convention demonstrations were solely an NAACP operation rather than a combined effort of A. Philip Randolph, King, and the association, and one that the NAACP entered into only reluctantly. In another gross omission, a discussion of the effort to save the jobs of African-American locomotive firemen, Watson never once points out that Randolph founded the Organization to Save Colored Locomotive Firemen's Jobs. Nor, for that matter, does he credit Randolph's more influential March on Washington Movement of the early 1940s as precedent, along with CORE, for King's nonviolent civil disobedience.

Watson's analysis of the role played by presidents Dwight Eisenhower and John Kennedy is confusing; in one place their efforts are praised while in another they are condemned for insufficient concern for civil rights. Moreover, his admiration for President Lyndon Johnson is such that he thinks it necessary to cover up the role played by the NAACP in Johnson's election in 1964. Watson insists that the association remained nonpartisan in national politics until the second Nixon administration in 1972, consequently

neglecting to mention the role of the NAACP in spearheading the 1964 Moratorium on Demonstrations. Nevertheless, because of the wealth of detail it provides on the passage of the civil rights laws, Watson's book will be a necessary starting place for any future history of the movement or biography of Clarence Mitchell.

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DONALD G. MATHEWS and JANE SHERRON DE HART. *Sex, Gender, and the Politics of ERA: A State and the Nation*. New York: Oxford University Press. 1990. Pp. xv, 283. \$24.95.

This book is a fine-grained case study of a decade of political conflict in North Carolina—one of three or four states that wavered on the brink of ratification of the Equal Rights Amendment (ERA) until the time ran out. A detailed political history serves as the backdrop to Donald G. Mathews and Jane Sherron De Hart's brilliant analysis of the rhetoric and political styles of proponents and opponents of ERA. In the process they demonstrate the serious consequences of the absence of a shared understanding of public life that makes possible healthy political conflict and debate.

Mathews and De Hart's analysis clarifies the underlying similarities of ERA proponents and opponents. Both groups, for example, were profoundly aware of gender-based inequalities. They differed, however, in their perceptions of women's public roles and as a result they spoke such different—often apocalyptic—languages that neither side could comprehend the other.

Whereas proponents brought positive experiences of public activity—through voluntary associations and professional work—opponents drew primarily on private identities, entering public life only in defense of such identities. Proponents believed they were involved in rational discourse where "facts" could be marshaled to support arguments based on the logic of individual rights. They clashed headlong, however, with opponents whose experiences of power were private and communal and who feared being forced to compete with men on an essentially male playing ground with male rules and norms. The rhetoric of individual rights simply made no sense to them.

Mathews and De Hart describe opponents as "cultural fundamentalists" who believed in fixed and universal patterns of behavior. The arguments by anti-ERA groups in favor of maintaining traditional gender roles eerily, and not accidentally, echoed earlier segregationist arguments about race. The authors' analysis illuminates the continuing power of politicized cultural identities and the fierceness with which people cling to them. We live in a world in

which the premodern, the modern, and the postmodern all collide.

This book adds to a growing body of scholarship on the struggle for the ERA, but it moves beyond "why did it fail?" to a cultural and political analysis of the meaning of the struggle itself. In this sense it is closer in spirit to Faye Ginsburg's study of the anti-abortion movement (*Contested Lives: The Abortion Debate in an American Community* [1989]) or Judith Stacey's ethnographic study of working-class families (*Brave New Families: Stories of Domestic Upheaval in Late Twentieth Century America* [1990]). Each traces a deep fault line of difference among white women that has to do with both gender and class identity and poses a set of fascinating problems for ongoing debates within feminist theory over "sameness" and "difference." Mathews and De Hart, however, use this debate to explore a central problem in the history of American public life.

What does it mean to bring identities rooted in private into public and political activity? What place does a gendered (or a racial) self have in public? And how can we create a renewed public discourse in which differences provide the beginning for conversation and debate rather than closing it off? Mathews and De Hart's study illuminates the problem in ways that point both to the possibility of richer historical analyses of many political conflicts and to more informed discussions about possibilities of public life in the contemporary world.

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CARLOS MUÑOZ, JR. *Youth, Identity, Power: The Chicano Movement*. (Haymarket Series.) New York: Verso. 1989. Pp. xv, 216. Cloth \$50.00, paper \$17.95.

Carlos Muñoz, Jr., offers us an insider's perspective on the Chicano student movement. From the Los Angeles Blowouts of 1968 to the demonstrations about Alan Bakke's reverse discrimination case of the late 1970s, Muñoz describes the men, the ideas, and the organizations. Although focusing primarily on California, he does give a sense of the regional breadth of Chicano activism from *La Raza Unida* in Texas to the Crusade for Social Justice in Colorado. He also delineates the connection between student protest and the institutionalization of Chicano studies. To this day, the political litmus test for Chicano academics is their past affiliations with *el Movimiento*.

The theme of student empowerment runs throughout the narrative. Small in number and first-generation college students, Chicanos found a home in the movement. They supported one another and articulated a commitment to the communities from which they came. The idea of returning to the barrio with political and administrative skills resonated throughout the student population. Muñoz lovingly

recalls a generation who saw a world beyond campus gates. He details the emergence of Chicano nationalism as both an amorphous ideology and practical strategy. It certainly lays to rest the notion that agitation by Mexican students developed as a "me, too" response to the successes of African Americans.

Chicanos have rarely appeared in histories of student protest and civil rights. Muñoz is at his best in his scathing critique of the recent outpouring of 1960s memoirs and monographs in which Mexicans are either ignored or relegated to a footnote. Through example after example, from the Chicano Moratorium against the War to the Third World Student Strike at Berkeley, the author recounts the integral roles Chicanos played in the antiwar and free speech movements. After reading this book, one is struck by the historical amnesia rampant among New Left scholars.

Muñoz, however, suffers from his own loss of historical memory. When relating the idealism, conflicts, and achievements of Chicano students, he means just that—the masculine ending—Chicanos. In two paragraphs, Muñoz relates that women held leadership positions and that a few challenged sexism within the student groups, but this is the extent of his analysis. Out of 212 pages, women appear eight times. He mentions two of the most well-known activists, Elizabeth Martínez and María Varela, almost in passing. As veterans of the Student Nonviolent Coordinating Committee, Martínez and Varela brought extensive political experience and organizing savvy to a diffuse and at times contentious movement. Their stories, as well as those of their *compañeras*, deserve a hearing.

The silence and downright hostility with which women's issues were received led to the creation of Chicana feminist organizations, many of which still exist. In resisting the nationalist message to stand by your man, Chicanas who fought for the inclusion of feminist goals found themselves castigated or ignored. It seems ironic that one of the first books on the Chicano movement would replicate the silence on gender. Indeed, in this work Muñoz has reconstructed *el Movimiento* with all of its rhetoric, promise, contradictions, and conflicts.

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RONALD P. FORMISANO. *Boston against Busing: Race, Class, and Ethnicity in the 1960s and 1970s*. Chapel Hill: University of North Carolina Press. 1991. Pp. xvi, 323. Cloth \$34.95, paper \$12.95.

It is hard to imagine that more than fifteen years have passed since Boston's busing controversy captured national attention. If the passage of time has robbed the issue of its immediacy, then Ronald P. Formisano has benefited from the perspective afforded by the

chronological distance. He has produced a nuanced treatment that is distinguished by its examination of context and telling detail even as it remains less burdened by them and more focused than its outstanding chief competitor, J. Anthony Lukas's Pulitzer Prize-winning *Common Ground* (1985).

Formisano rejects the notion that the busing dispute was nothing more than a morality play or that the issue could be ineluctably reduced to "racism." Racism there surely was, but Formisano's exploration of ethnicity, religion, class, and turf adds texture to what was anything but a one-dimensional phenomenon.

In labeling the confrontation over busing "a war nobody won" (p. 203), Formisano conveys a sense of tragedy. The war described in these pages, however, was at least as much an intrawhite dispute as an interracial conflict. Not only are the familiar divisions separating liberal, cosmopolitan elites and localistic, ethnic, working-class conservatives outlined but the author also scrutinizes the antibusing opposition, revealing not a monolithic, "racist" mass, but a large group of "moderates" and a wide range of reactions. Terrorism and intimidation within the white community crushed dissent in the most bitterly contested neighborhoods and conveyed the image of unanimity where, according to Formisano, diversity was the rule. Surely there is little argument with his central conclusion. By the 1980s Boston's public schools were more segregated than ever, now by both race and class, and quality educational reform—never a central concern for either busing's advocates or opponents—remained as elusive as ever.

Culpability is spread around. Judge Arthur Garrity is taken to task for being both too harsh and too lenient. The pairing of Irish South Boston and black Roxbury high schools, the adoption of successive plans that fostered instability, and the rejection of a less-disruptive approach that might have garnered public support all strike Formisano as judicial blunders. Yet, in the author's view, Garrity's refusal to hold opposition leaders in contempt sustained the antibusing movement. His "expert" advisers are similarly chastised and those politicians and suburban-based city elites who remained aloof are also held accountable. Still, the greatest measure of responsibility for Boston's time of troubles is assigned to "the anti-busing political entrepreneurs who promised 'never' and then exhorted their frustrated followers 'to stick together and fight'" (p. 218). If the proposed remedy was less than ideal, the cynical racial manipulations of the Boston school committee and the self-serving posturing for personal and political ends on the part of the most vocal opposition proved destructive.

Particularly strong in placing Boston's besieged ethnic neighborhoods in a context that stretches back to the era of urban renewal and ranges beyond race, *Boston against Busing* nonetheless raises some questions. Having written a much more hopeful book

than Lukas, Formisano's empathy for and emphasis on the white "moderates"—combined with the dissection of Garrity's tactical errors—implies that a different plan would have produced different results. Perhaps. But the author's "moderates" themselves comprise a loosely defined, diverse group whose "moderation" is sometimes evident only when juxtaposed against the backdrop of brick-throwing hooligans. The real tragedy, of course, is that we will never know.

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N. N. BOLKHOVITINOV. *Russko-amerikanskii otnosheniia i prodazha Alaski 1834–1867* [Russian-American Relations and the Sale of Alaska, 1834–1867]. Moscow: Nauka. 1990. Pp. 368. 3 r.

This admirable book, with its thorough research and careful interpretation, caps more than three decades of systematic study of (and some forty articles and several monographs on) the history of Russian-American relations from the American revolution through the American Civil War by the foremost Soviet specialist on the subject, Nikolai Nikolaevich Bolkhovitinov of the Institute of General History of the Academy of Sciences of the USSR. He has mined both Soviet and American repositories and placed the sale of Russian Alaska (if not its purchase) firmly and fully in the context of Russian-American relations after the expiry of the ten-year Russian-American Convention of 1824. His use of Soviet archives in particular (until recently largely closed to Western researchers) will make this study authoritative, if not definitive.

Bolkhovitinov is bluntly critical of some earlier work by Western and Soviet scholars alike who have neglected the archival sources and made facile judgments. He draws a number of major conclusions. In the North Pacific in the second third of the nineteenth century, relations between Russia and the United States—both expansionist powers—were characterized by competition and cooperation alike, with neither generally dominant (and their relations did not deteriorate until the 1880s). On the eve of the sale of Alaska, the economic position of the Russian-American Company, the colony's monopolistic administrator and exploiter, was not critical (indeed, it was still quite profitable). The external threat, or the indefensibility of Russian America in case of war, was an important but not a decisive reason for the sale; American expansion, although more potential than real, was taken into consideration by Russian decision makers. More general reasons for the cession included the restrictions of Russian serfdom (even after emancipation in 1861), the small Russian population of the colony (never more than about eight hundred persons), and northwest coast Indian (particularly

Tlingit) resistance to the Russian presence; these reasons are general because they do not rate "particular deliberation" in the documents. The primary reason for the disposal was the desire to eliminate a possible source of future conflict between St. Petersburg and Washington and to forge an actual alliance (against Great Britain in particular) between the two countries (thus, in July 1868 Congress decided to ask the president to hasten the lifting of all obstacles to "the passage of merchant and naval vessels through the Straits [between the Mediterranean and Black Seas]," one of the main objectives of Russian foreign policy in the aftermath of the Crimean War). Another decisive factor was the shift in Russia's Pacific interest to the consolidation of its position in the Far East, especially in the Amur basin.

The author points out that, by virtue of the sale, the Russian empire became the first European colonial power to relinquish voluntarily its overseas territory—a decision that he sees as the logical outcome of an old outlook. As early as 1788, in reply to a request for governmental assistance from the Golikov-Shelikhov Company (one of the private forerunners of the Russian American Company), Empress Catherine II had declared that "the [Russian] American settlements are not flattering models, especially as the lands are not advantageous to the motherland. . . . Further expansion in the Pacific Ocean will not bring solid benefits" (p. 317). To trade was one thing, to occupy was quite another. Bolkhovitinov concludes: "The old notion of the continental rather than maritime future of Russia, the repudiation of far-away, overseas territorial acquisitions, and the concentration of attention on the strengthening of its position in the Far East, Siberia, and Central Asia became more and more important" (p. 317).

The volume is endowed, fortunately, with footnotes instead of endnotes (would that Western publishers were as concerned about book users), numerous miniature portraits of policy advisers and makers, an appendix of nine key documents, a table of contents in English as well as Russian, and a bargain price tag. Regrettably, the paper and binding are substandard, the few tables are untitled, there are no maps, and the index contains personal names only. These minor shortcomings aside, this fundamental study richly deserves what has already happened to not a little of the author's work: translation into English for a much wider, particularly American, audience.

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LESTER D. LANGLEY. *America and the Americas: The United States in the Western Hemisphere*. (The United States and the Americas.) Athens: University of Georgia Press. 1989. Pp. xxii, 307. Cloth \$30.00, paper \$15.00.

Lester D. Langley, a long-time scholar of U.S.-Caribbean relations, has written a survey of Latin American-United States relations from independence to the era of Ronald Reagan. He examines these relations from two rather different perspectives: the idea of North American hegemony and the idea of Latin America as a distinct culture, unamenable to the imposition of U.S. institutions and values. As he proceeds through this dual analysis, Langley also separates the nation-states of the New World from the concept of "America." He sees "America" as the repository of the ideals of hemispheric cultural unity based on the newness and the heterogeneity of its societies. In particular, Langley makes an effort to distinguish between the two roles played by the United States. On the one hand, it is a selfish, at times aggressive and hegemonic nation-state; on the other hand, it is "America," whose people are sympathetic to ideals of regional community that transcend Anglo-Saxon culture. Despite his attempt to break new ground, however, Langley's historical survey seems confined within the classic dichotomy of ideals versus self-interest, except that the places of these terms are now taken, respectively, by "America" and the "United States."

Even in terms of the old dichotomy, few new insights are developed. At many points, for example, Langley finds U.S. policy determined by concerns of national security. He is usually unhappy with such policies but finds them "understandable" (p. 260). Nowhere in the book does the author attempt to define national security, nor does he challenge the various claims that are made for it by U.S. leaders. When discussing the war against Augusto César Sandino, he finds "indisputable" [U.S.] security interests in Nicaragua" (p. 130). The reader is left with the impression that national security is a solid, identifiable interest trailing no connections to the nation's ideals. These ideals are dispatched to the airy realm of ideology. Contemplating the cautious U.S. attitude toward the independence of Latin America, Langley concludes that the United States "placed national interest before ideological commitment" (p. 38).

Langley seems not only unable to probe the sources of U.S. hegemony, he also seems uncomfortable with his professed respect for the genius of Latin American culture. He finds that the policeman role of the United States in the Caribbean before World War II was stymied by "resilient cultures with their own priorities, their own way of doing things, and their own unappreciated strengths" (p. 132). But these are the final words of the chapter. The reader never learns what these "priorities" (surely not a Latin American concept) and strengths might be. The author discusses the problem of Latin America in the 1980s as one of the "survival of democracy" amid economic decline (p. 250). Moreover, he finds that Latin America's "statist economies have inherited defects" (p. 254).

The author suggests that wise North American

policy will be the work of enlightened leaders rather than of the general citizenry and worries about the "irrational and emotional instincts of the general public" (p. 262). He has chosen his adjectives unwisely because these are terms for expressing North American cultural disdain of Latin America. Nor do the masses of Latin America seem to figure prominently in the task of escaping from their present state. Langley says simply that the "ordinary people" are seeking a "different future" (p. 265). In the 1980s, he finds that "those who ruled [Central America] failed to produce a more democratic political and social order" (p. 237). Quite so, but the reader is not told why a "democratic political and social order" was to be expected from them.

Langley is to be commended for seeking a new vantage point on Latin American–United States relations, one that grants a greater historical autonomy to Latin America and dispenses with the sterile insistence that the role of the United States is either the problem or the solution. His efforts might have been better rewarded had he been able to put old perspectives wholly behind him.

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RICHARD H. COLLIN. *Theodore Roosevelt's Caribbean: The Panama Canal, the Monroe Doctrine, and the Latin American Context*. Baton Rouge: Louisiana State University Press. 1990. Pp. xviii, 598. \$45.00.

Within the context of his own time, Theodore Roosevelt was a pragmatic politician concerned predominantly with the exclusion of Europe from Latin America rather than the subjugation of southern neighbors. Although "historians . . . are especially and almost universally critical of his Panama and Caribbean policies," a careful study of Roosevelt's handling of crises in Panama, Cuba, Central America, and the Dominican Republic convinces Richard H. Collin that Roosevelt has been misjudged by historians (p. 546).

The bulk of this lengthy, generally well-written study focuses on Roosevelt's handling of the crisis at Panama in 1903 (212 pages) and in the Dominican Republic in 1904–05 (122 pages). Collin provides far less coverage of events in Central America (36 pages) and Cuba (42 pages).

Examining the Dominican Republic's debt crisis at the turn of the century, Collin concludes that Roosevelt acted sensibly in establishing a customs receivership in that troubled nation. Similarly, Collin reasons that Roosevelt's handling of the canal issue reveals a practical leader whose proposals to Colombia were quite reasonable. The critical problems that emerged in the Colombia crisis can be attributed more to a "determinedly uncooperative" José Marroquín administration in Bogotá and to ultranationalist

U.S. senators in Washington, D.C., than to Roosevelt (pp. 193, 311).

Collin's conclusions support those of Frederick Marks (*Velvet on Iron* [1979]) and contribute to an emerging neoconservative interpretation of early twentieth-century U.S. diplomacy that downplays or rejects the image of Theodore Roosevelt and the United States as imperialistic. For a distinctly different interpretation of the Panama crisis, for example, compare Collin and Marks with my own work (*Arrogant Diplomacy* [1987]) and Walter LaFeber (*The Panama Canal* [1978; rev. ed., 1989]).

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EDWARD M. BENNETT. *Franklin D. Roosevelt and the Search for Victory: American-Soviet Relations, 1939–1945*. (America in the Modern World.) Wilmington, Del.: Scholarly Resources. 1990. Pp. xxvii, 207. Cloth \$40.00, paper \$13.95.

In the first volume of this study of Soviet-American relations from 1933 through 1945 (*Franklin D. Roosevelt and the Search for Security* [1985]), Edward M. Bennett portrayed a well-intentioned Franklin D. Roosevelt whose vacillating and unrealistic diplomacy contributed to the estrangement between Moscow and Washington. He picks up the same theme in this new volume, a well-researched, if narrowly conceived, brief survey of the Soviet-American connection during World War II.

Relying heavily on archival materials, the author has written an old-fashioned diplomatic history that rarely leaves the Roosevelt Library and the National Archives for a glance at the larger picture. Moreover, although Bennett's analysis is informed by recent Soviet scholarship, particularly discussions at the American-Soviet Colloquia at Moscow and Hyde Park in 1986 and 1987, he concentrates on Roosevelt and his advisers.

Bennett's Roosevelt frequently seemed to be moving in the right direction—a realistic appraisal of Soviet national security needs—but over and over, he either surrendered to the hard-liners in his entourage or to his dangerously naive Wilsonian image of the postwar world. Further, the often capricious president careened from crisis to crisis, displaying little interest in long-range planning. Finally, he made it difficult for American diplomats to understand what their boss was up to by constantly shifting policies that were held close to his vest. Bennett also censures the insular and insensitive Joseph Stalin for not understanding that American politics necessitated a more compromising posture from his camp.

Because both sides clung to their principles and eschewed serious negotiations on substantive issues, chances for postwar rapprochement were slim. Bennett is not the first to conclude that the worst of the cold war might have been avoided had the United

States agreed to an amoral geographic settlement in 1943 or 1944.

The book's limited focus, as well as its brevity, makes it difficult to accept such a conclusion without more analysis of domestic politics, public and elite opinion, military factors, economic planning, and views from other capitals. In addition, Bennett almost completely ignores revisionist and postrevisionist monographs and important studies of the role of Great Britain in influencing American policies. Thus, absent from the bibliography—as well as the narrative—are the contributions of such authors as John Lewis Gaddis, Gabriel Kolko, George Herring, Thomas Paterson, Geir Lundestad, Frederick Marks III, Daniel Yergin, William Taubman, William O. McGagg, Terry H. Anderson, and Robert Hathaway.

Considering the limited task implicitly defined by the author, Bennett's execution is sound, aside from some disconcerting chronological meandering at the outset. The book is a useful introduction to the major conferences and policy decisions in Soviet-American relations during World War II. In addition, Bennett's main theme, that of missed opportunities, is an important one to which we will no doubt return as the Russians continue to open their archives.

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NATALIE HEVENER KAUFMAN. *Human Rights Treaties and the Senate: A History of Opposition*. Chapel Hill: University of North Carolina Press. 1990. Pp. x, 256. \$32.50.

This book presents an analysis of the U.S. Senate's attitude and actions regarding human rights treaties since World War II. Natalie Hevener Kaufman is concerned with investigating the reasons why the United States has ratified only a few of these many treaties. She focuses on the arguments and tactics used to defeat these treaties, a legitimate methodology because the opposition has so often won.

The opposition's first opportunity was the Genocide Convention, adopted by the United Nations General Assembly in 1948 but not ratified by the Senate until 1988. Among the groups opposed were a section of the American Bar Association (ABA), the most conservative senators, and several of the usually self-styled "patriotic" societies. These groups could successfully play on the fears prevailing in the late 1940s and 1950s: international communism, world government through the United Nations, racial segregation. The arguments frequently used against the treaties were that they diminished basic rights, narrowed states' rights, advanced world government, expanded communist influence, subjected U.S. citizens to trial abroad, and limited American jurisdiction. Although gradually those fears diminished and the ABA eventually changed its position, the author demonstrates convincingly that most of the arguments remain influential. Such arguments serve the

cause of conservatism, and the same groups that successfully oppose these international treaties may also usually be found among opponents to any liberal or progressive measure within the United States. Moreover, Kaufman states that the prospects for ratification of human rights treaties are dim given the Senate's general resistance to change. Some hope exists that the substance of human rights treaties that are not already part of the American legal system (or foreign policy) may become so when it turns into international customary law. This consideration, although beyond the author's concern, is offered here as a possible happy ending to an otherwise disheartening, yet very well-told, story.

The author's style is fluent and her case fully substantiated. Because the opposition's arguments do not change significantly over time but the context does, there is, probably inevitably, some repetitiveness. This survey is useful because the strong impact of the opposition is impressively demonstrated and, in my opinion, remains to be counteracted before the United States can become a participant in the efforts to make human rights each nation's obligation toward the international society.

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A. F. K. ORGANSKI. *The \$36 Billion Bargain: Strategy and Politics in U.S. Assistance to Israel*. New York: Columbia University Press. 1990. Pp. xvii, 315. \$30.00.

All too often, scholarly analyses of U.S.-Israeli relations devolve into polemical treatises that abandon balance and impartiality. A. F. K. Organski, a political scientist, struggles to avoid this fate but only partially succeeds. This volume is always engaging in its argumentation, sometimes insightful in its exposition, but frequently unconvincing due to the one-sided approach that leads the author to ignore some evidence and draw questionable conclusions.

Organski's primary thesis is that large-scale U.S. support for Israel is based on newly perceived national security requirements adopted by President Richard Nixon around 1970. Pro-Israeli lobbying efforts and the political activities of America's Jewish community are viewed as essentially unimportant factors leading to the closer alliance. Israel is such a valuable ally, the author insists, that the \$36 billion in assistance has been a bargain compared to other national security expenditures.

Aid to Israel after 1970, Organski argues, was a logical response to the Soviet Union's military expansion and widening influence in the Middle East. U.S. goals in the region—exclusion of further Soviet influence, access to oil, a check on the radical Arab regimes that were being armed by the Soviets—led Nixon and subsequent presidents to rely on Israel to implement desired policies. Israel acted more aggres-

sively than the United States dared to contemplate acting; yet Israeli boldness, such as the 1981 destruction of Iraq's nuclear reactor, served U.S. interests.

The author is so determined to establish Israel's credentials as a valued national security partner that he fails to acknowledge that a pro-Israeli constituency in the United States has also played a significant role in ensuring that the two nations are extremely close. Since the mid-1970s, Israel has received 25 percent of all American foreign aid. Many nations are important to U.S. security, but among them only Israel possesses the lobbying capability to achieve its military and economic aid requirements by relying on domestic U.S. political processes.

Organski argues that pro-Israeli interests did not directly influence U.S. policy because Israel received relatively little economic and military assistance from 1948–70, and a lot after 1970. There were just as many Jews, they were concentrated in states critically important in presidential elections prior to 1970, and yet they did not have much power before 1970 because Israel received little aid before 1970. Why should it be assumed that pro-Israeli forces suddenly were able to wield political influence after 1970? According to Organski, what occurred after 1970 is that U.S. leaders realized that it was in the nation's security interest to align itself closely with Israel.

The fallacy in Organski's analysis is that he establishes one criterion—the level of economic and military assistance that the U.S. gives to Israel—to judge the relationship over four decades. Israel was not in the market for large-scale foreign aid assistance, which has been largely military, until it became a necessity in order to match the Soviet buildup of its Arab clients after the Six Day War in 1967. Indeed, Israel's military performance in the 1956 and 1967 conflicts dispel the notion that there was a need for massive military assistance.

Actually, Israel fared quite well in the years before 1970 in obtaining from America that which it wanted. During the period from the creation of Israel in 1948 until 1970 the American Jewish community established itself as a formidable force in influencing foreign policy in the Middle East. The administrations of Harry Truman, John Kennedy, and Lyndon Johnson were fully pro-Israel. Organski frequently cites Eisenhower's cool relationship with Israel; Eisenhower was the exception, however, among the presidents.

Eisenhower had broad national support throughout his two terms, he had received few Jewish votes, and he was therefore relatively immune to the influence of pro-Israeli political forces. Truman, Kennedy, and Johnson were not in a position of such political independence when issues arose concerning Israel; in the past half-century, no Democratic candidate has won the presidency without receiving at least 70 percent of the Jewish vote.

Because it runs counter to his theme that American support for Israel is based on security interests,

Organski consistently overlooks the evidence that U.S. support is also generated by a strong pro-Israel lobby and the political influence of American Jewry. The American Israel Public Affairs Committee (AIPAC), widely considered the most effective foreign policy lobbying force in Washington, is dismissed from consideration with the comment that evidence for and against its power is "entirely anecdotal or downright gossip" (p. 19).

Sometimes his evidence fails to support his conclusions. For example, Organski's detailed study of U.S. Senate votes purports to show that Jewish voters do not have much influence on how their senators vote on issues of interest to Israel. The data, however, shows the opposite. In thirty-three states where Jews make up less than 1 percent of the population, senators supported pro-Israeli positions 63 percent of the time. This pattern is consistent with Organski's contention that a pro-Israeli vote can come from a representative whose interest is in securing America's national security requirements. The data, however, also indicate that senators with a Jewish constituency of 3 percent or more of the population voted for Israel 93 percent of the time. Surely this figure is strong evidence of the political clout of American Jews on issues involving Israel.

Probably the most successful element of the book is the clarity with which it places in perspective the direct aid given Israel in comparison with the indirect assistance given to other U.S. allies. On one hand, Israel acts as a proxy for the United States in many instances; Israel has been hired, it could be said, and America sends over the economic and military resources so Israel can do the job. On the other hand, the overhead costs of the security structure for the American international commitment has been somewhere around \$3 trillion. When the expenses of NATO, maintaining American troops around the world, fighting wars, and other costs are factored in, the commitment to Israel does not appear too dramatic. Organski persuasively notes that Israel has received 2 percent of the total U.S. cost of being a world power over the past forty-five years.

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San Luis Obispo

CANADA

JOY L. SANTINK. *Timothy Eaton and the Rise of His Department Store*. Buffalo: University of Toronto Press. 1990. Pp. xii, 319. \$35.00.

The study of business has been an important field in American history for many decades, but it is only in the past twenty years that it has become a significant field of inquiry in Canadian history. Scholarly books have appeared in steadily increasing numbers, and this study continues to follow this trend. It is the first scholarly biography of Timothy Eaton (1834–1907), a

Canadian businessman whose small Toronto dry goods store, opened in 1869, had by the 1890s become Canada's largest and most successful department store.

Joy L. Santink places Eaton's store in the context of the rise of large department stores in the United States, Britain, and France in the nineteenth century. Eaton's fits into this general pattern in several ways. All the department stores now in existence, including Eaton's, began as dry goods stores, with the exception of Harrod's in London. All these stores adopted two new business practices—cash sales and fixed prices—as did Eaton's in Toronto. Other major innovations were the gradual introduction of a wider range of merchandise, the creation of separate departments for each product, and the policy of rapid turnover of stock.

The author demonstrates, however, that there were also unique reasons for Eaton's success in Toronto. Many of his British and North American contemporaries catered mainly to the middle class and the emerging white-collar workers. Eaton's, on the other hand, appealed primarily to the working class that was increasing substantially as Toronto underwent rapid industrialization from the 1870s on. The author makes a convincing case that Eaton's religious beliefs were essential to his success. He was a convert to Methodism, which stressed individual salvation, and his religious convictions suited his highly independent personality. He accepted competition as an essential feature of the retail trade and worked vigorously and shrewdly to outdo his rivals. I would like to know what importance the author places on family connections as a factor in Eaton's success. Surely it is significant that by 1892 five of his sons and nephews were on the managerial staff of Eaton's store and that his son succeeded to the presidency on Eaton's death in 1907.

Santink's study is a solid contribution to Canadian business history, a portrait of a businessman whose independent spirit and willingness to innovate revolutionized the retail trade in Canada and made him one of the major Canadian businessmen of his time.

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LATIN AMERICA

LAIRD W. BERGAD. *Cuban Rural Society in the Nineteenth Century: The Social and Economic History of Monoculture in Matanzas*. Princeton: Princeton University Press. 1990. Pp. xxi, 425. \$65.00.

Beginning in the eighteenth century and continuing through the twentieth century, noteworthy differences evolved in Cuba's six provinces to distinguish each from one another. Some of these differences originated early in the century of European discovery and were related to the character of the conquest and colonization, which in turn shaped the course of

settlement and migration. Other differences stemmed from local geography: soil types, mountains, rivers, and the availability of harbors. Caribbean winds and gulf currents also accounted for differences among the provinces.

These factors established the potential for other types of developments, many of which served to accentuate greater differences. Perhaps nothing shaped these distinctions as profoundly, or as remorselessly, as sugar production in the nineteenth century. Sugar changed everything, not necessarily all at once, and not always in the same way, but always in a manner that was far reaching and often in ways that were inevitable and permanent.

Scholars of Cuba have long been generally familiar with these developments. These notions are very much part of provincial lore and have become the conventional historiographical wisdom. Such general familiarity has not, however, always served the best interests of new scholarship. Indeed, the details of the changes wrought by sugar in the nineteenth century have often been lost or otherwise obscured in the historical literature derived from this familiarity.

Scholars of Cuba will long be in the debt of Laird W. Bergad for a lucid and penetrating study of Matanzas province during the nineteenth century. No previous study has provided an analysis so sweeping in scope and so rich in detail. Bergad offers a compelling analysis of the changes that transformed Matanzas and thus made this province so very much unlike the other five. Nowhere in Cuba during the nineteenth century did the sugar revolution advance as far as fast and with such profound consequences as in Matanzas. The Matanzas sugar transformation, as Bergad convincingly demonstrates, was cause and effect of other changes, no less far reaching. The transformation implied new uses of capital, technology, and land tenure. It altered banking and finance and income distribution, and modified production strategies. Sugar summoned into existence a vast labor system based on chattel slavery, which in turn transformed race relations and class structures. Sugar shaped local commerce and international trade, both of which served to integrate Cuba into the North American economy, with long-lasting effects. Marriage strategies, consumption patterns, and vacation itineraries also changed. Sugar shaped the ways Cubans made war in 1868–78 and again in 1895–98, and the way they made peace. In other words, sugar production was all-encompassing and, as Bergad demonstrates methodically and systematically, virtually no aspect of provincial life escaped unaffected by sugar.

Bergad provides more than a detailed provincial study. The focus on a single province should not obscure the national and international implications of developments in Matanzas. Bergad is careful to integrate Matanzas into the larger Cuban context, which in turn is linked to the global economy. He also offers a constellation of analytical elements with which to

focus attention on other provinces in Cuba, as well as other sugar zones of the New World. In the finest scholarly tradition, Bergad has provided a model study that will serve as a point of departure for future research.

LOUIS A. PEREZ, JR.
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JOHN K. CHANCE. *Conquest of the Sierra: Spaniards and Indians in Colonial Oaxaca*. Norman: University of Oklahoma Press. 1989. Pp. xvii, 233. \$34.50.

John K. Chance's earlier work on the society of the colonial city of Oaxaca in southern Mexico (*Race and Class in Colonial Oaxaca* [1978]) is well known. Now he has extended his research into the backlands of rural Oaxaca, examining the economy, society, and politics of a remote mountainous area, the *alcaldía mayor*, or local administrative district, of Villa Alta.

Chance's accounts of the Spanish conquest and settlement of Villa Alta, of its demographic history, and of its colonial economy, give valuable new information, but offer few surprises. The author succeeds most impressively, though, in the final two chapters, on native community organization and religion. Here the analysis becomes denser and more nuanced.

In some major respects the colonial history of Villa Alta conforms with that of the valley of Oaxaca, the large and fertile farming region surrounding the city. The pioneering work of William Taylor showed twenty years ago that native society and landholding endured in the valley throughout colonial times to a degree unknown in other parts of Mexico. The same survival of pre-conquest patterns marks the Villa Alta region. The land was poor and the native people were comparatively backward. No Spanish estates developed and Spanish population remained very small.

Yet in several respects the impact of colonial rule was severe. Indians were relocated in a series of *congregaciones*, advocated especially by the Dominicans, who were the main evangelizing force up to 1700. Native religion persisted strongly until then, although it was much weakened in the eighteenth century by secular clergy avid to suppress idolatry. But "pagan" elements carried over into the cult of saints that became the prevalent form of communal spirituality after 1700—a syncretic folk Catholicism.

Equally telling was the economic effect of colonial rule, and its social outcomes. Paradoxically, even though the Villa Alta area had few attractions for Spanish settlers, it proved, especially in the eighteenth century, immensely profitable to its colonial administrators. Chance elucidates in detail the mechanism of forced sale of goods to Indians, and forced production of commodities—the notorious *repartimiento de efectos*—that brought these gains to the *alcaldes mayores* from about 1650 onward. State power was used to force Indians to produce cochineal and to weave cotton cloth that the *alcaldes* then sold at a

profit. To facilitate this extraction of wealth, the *alcaldes* employed native leaders as brokers—not the traditional *caciques* of the communities, but a new group of authoritative figures who had risen, partly, at least, in the alien system of civic government by *cabildo* that the Spaniards had begun to impose around 1560.

Chance intricately traces the sequences of cause and effect linking resources, pre-contact economic and social structure, native and imported religion, newly imposed administrative practices, and local and external demand for the products of Villa Alta. Readers will find his arguments requiring and deserving their concentrated attention. They will also be impressed by his demonstration that even such a remote region as this can be closely studied for the whole colonial era. Local sources are few and topically limited, but they yield richly here to a keen interpretive intelligence.

PETER BAKEWELL
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FRANS J. SCHRYER. *Ethnicity and Class Conflict in Rural Mexico*. Princeton. Princeton University Press. 1990. Pp. xviii, 363. \$45.00.

Agrarian revolution came late to Huejutla, Hidalgo, Mexico: six decades after the Mexican revolution broke out in 1911, a group of poor peasants invaded a pasture owned by a rich Indian peasant in Pepeyocatitla. Over the next fifteen years, a number of other violent upheavals shook the region. Given the obvious injustice of the system of land tenure, why did it take so long? And what effect did the long-overdue eruptions have on the community and culture of Huejutla?

Frans J. Schryer finds in the Huejutla region a wide diversity of community and culture based on ethnicity (mestizo or Indian) and geography. He also sees a three-class structure in mestizo and Indian villages: an upper class of "rich" or "capitalist" peasants who employed wage laborers, and a few schoolteachers who dabbled in different economic and political pursuits; an intermediate stratum of small, basically self-employed cultivators, artisans, or skilled workers; and a lower class of day laborers who cultivated small subsistence plots. Most fascinating are his discoveries about land tenure. The seemingly clear-cut legal distinctions among the various types of proprietorship (*ejido*, small landholder, and communal) actually did not exist. Many of the reform measures taken before 1970 were fictitious. Local peculiarities persisted, despite national bureaucratic intervention, well into the 1980s.

The author debunks a number of traditional anthropological notions of peasants (although these are not necessarily widely accepted at present). Schryer finds, for example, that the concept of the closed corporate Indian community especially exaggerated

the views that class conflict is absent in Indian communities. He does argue for the remarkable persistence of Indian culture (in this case Nahua) and traditions.

Schryer concludes that calm prevailed in Huejutla until 1970 because of an "ecological equilibrium" produced by the stable coexistence of subsistence cultivation with commercial agriculture and cattle raising (p. 319). Low population density combined with swidden cultivation furnished even the poorest peasants with access to enough land to supply their subsistence. Slash and burn cultivation created pastures for cattle. Large landowners could hire cheaply as long as they provided easy access to subsistence plots. Paternalistic relations predominated. Wealth depended on the ability to attract a labor force, which in turn required promises of protection and access to land. This "moral economy" did not break down until the 1960s, lasting through the revolutionary civil wars (1911–20) and the reforms of Lázaro Cárdenas (1936–40). The introduction of modern cattle raising methods and a large increase in population in the 1960s, however, destroyed the old symbiotic relations. Unrest followed. The author is very careful to explain the chronological and geographical variations in these developments.

Schryer's book will be welcomed by those who believe that the Mexican revolution was a sham and that the current regime is corrupt and oppressive. Reform in the countryside had little to do with justice and much to do with politics.

MARK WASSERMAN
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New Brunswick

THOMAS E. CHAVEZ. *Manuel Alvarez, 1794–1856: A Southwestern Biography*. Niwot: University Press of Colorado. 1990. Pp. x, 243. \$24.95.

Manuel Alvarez was a Spaniard who emigrated first to Mexico during the Latin American wars for independence, then to Cuba and the United States briefly, and finally to New Mexico in 1824. There he spent the remainder of his life, except for occasional journeys to Missouri, New York, and Paris. Fluent in English, Spanish, and French, he became a traveler, fur trapper, stockraiser, U.S. consul at Santa Fe, judge, *político*, elected official, and, above all, an honest, well-respected, and successful merchant. After the Mexican-American War he served as New Mexico's lieutenant governor and acting governor and dedicated himself to statehood and self-government for New Mexicans.

Until now Alvarez has been little known in the history of the Southwest. Although historians have touched on his presence and some of his activities, only a monograph by the late Harold Dunham and an unpublished manuscript by Walter Sadlowski, Jr., have focused on this multifaceted New Mexican.

Thomas E. Chávez has corrected this neglect of Alvarez's career. Chávez's research is based on manuscripts in archival collections, government sources, contemporary newspapers, and a wide range of secondary works.

This is the first thorough, scholarly biography of the life and activities of Alvarez. Well organized and clearly written, the book contains an introduction, eight chapters, epilogue, chapter notes, selected bibliography, index, three maps, and twenty-one illustrations (none of Alvarez because none exist). Chávez covers in detail such topics as Alvarez's travels, citizenship problems, diplomacy, consular duties, promotion of New Mexico's statehood, relations with Anglos and Hispanos, and political difficulties in New Mexico that were evidently as tumultuous then as they are today. Throughout Chávez discusses Alvarez's merchant activities, the operation of his store in Santa Fe, and his far-flung business enterprises from Missouri to California.

No book is without its faults, however, and this one has a few. The statement that "New Mexico became an integral part of the 1850 Missouri Compromise" (p. 5) confuses the Missouri Compromise of 1820 with the Compromise of 1850, which created New Mexico's territorial status. The author's use of the term "Mexican Revolution" (p. 17) should refer to the war for independence, not the Mexican revolution, which began in 1910. A later statement that Texan independence was "complete with the Treaty of Velasco" (p. 34) is misleading because that treaty was never ratified by Mexico, which did not recognize Texas's independence. Finally, it should be noted that the battle of Resaca de la Palma (p. 102) did not occur on April 25, 1846, a date when Mexican forces attacked Captain Seth Thompson's troops north of the Rio Grande. The battle of Resaca de la Palma occurred on May 9, 1846. Despite these slips, this is an excellent, comprehensive, and readable account of the life of Alvarez, and it should appeal to a wide range of scholars and lay persons alike.

OAKAH L. JONES
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MARSHALL C. EAKIN. *British Enterprise in Brazil: The St. John d'el Rey Mining Company and the Morro Velho Gold Mine, 1830–1960*. Durham, N.C.: Duke University Press. 1989. Pp. xxiii, 334. \$47.50.

This work is a deftly crafted study about the operations of a British company, including its labor history, the evolution of its technology, and the broader social history of the Brazilian community where the foreign enterprise was based. St. John d'el Rey operated the largest gold mine in South America and was one of the most successful foreign enterprises in Latin American history, to which its 130-year life attests.

Marshall C. Eakin argues that the company, despite its enormous local importance, had rather little influ-

ence on the Brazilian state, although he notes that gold mining was not a leading export industry. In discussing the company's operations, the author displays an authoritative grasp of the technical features of gold mining and relates them skillfully to labor requirements.

Before its abolition in 1888, slavery was an important feature of production for the St. John. After 1850 the majority of bondsmen were rented for use in this industrial enterprise: the Aberdeen Act of 1845 prohibited British subjects from owning slaves, but the St. John company lobbied successfully to permit Britons to hire slaves and to retain those whom the company had purchased before 1845. The rental system was far from an impediment to the firm. Eakin notes: "the St. John was able to hire slaves in the peak of their productive years, and to discard them at no cost as they became less productive" (p. 34).

In part two of the book, a study of the company town Nova Lima, Eakin establishes the ways in which the community's social features were shaped by the fortunes of the St. John. Unfortunately, records on the company's slaves are limited, but the author provides descriptions of daily life, slave ethnicity, and techniques of social control, including manumission. Eakin shows how a working class, derived from the local peasantry, rose to replace the slave regime, and, in the decade after World War II, how labor first

challenged the company, but then collaborated with it as financial difficulties ensued. Nonetheless, the company failed, and the St. John's mine at Morro Velho passed to a Brazilian owner in 1960.

The chapter on the unassimilable British "elite" in the community is rich in detail and provides extensive evidence of the social dualism that was a peculiar feature of this type of enterprise. The local Brazilian elite provided a political buffer for the company, eventually becoming dependent on it as wholesale suppliers.

The role of foreign firms in Latin America's economic development has been the subject of heated controversy and the author feels obliged, like so many before him, to relate his monograph to dependency theory. Yet, unlike most writers who address this problem at the case-study level, Eakin refreshingly gives a qualified endorsement to the approach of Fernando Henrique Cardoso and Enzo Faletto, who assign primacy in situations of dependency to local and national dynamics rather than to international forces (p. 268).

Whatever its implications for social theory, this important study successfully combines business history with local social history, revealing in the process much about Brazil at large.

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Collected Essays

These volumes, recently received in the AHR office, do not lend themselves readily to unified reviews; the contents are therefore listed.

JOCK PHILLIPS, editor. *New Worlds? The Comparative History of New Zealand and the United States*. (The Fulbright Seminars.) Wellington, N.Z.: Stout Research Centre, for the New Zealand-United States Educational Foundation. 1989. Pp. 176. \$NZ 16.00.

JOCK PHILLIPS, Introduction. RAEWYN DALZIEL, Women in New Societies. DONALD HARMAN AKENSON, Immigration and Ethnicity in New Zealand and the U.S.A.: The Irish Example. THEDA PERDUE, Native American Revitalization Movements in the Early Nineteenth-Century. DAVID HAMER, Towns in New Societies. ERIK OLSEN and JEREMY BRECHER, New Zealand and United States Labour Movements: The View from the Workshop Floor. TOM BROOKING, Superficial Similarities: Why Did Farmer Protest Assume Such Different Forms in Late Nineteenth-Century New Zealand and the U.S.A. PETER J. COLEMAN, New World Political Cultures.

ELISE MARIENSTRAS, editor. *L'Amérique et la France: Deux révolutions*. Foreword by CLAUDE FOHLEN. (Série internationale, number 39.) Paris: La Sorbonne, in association with the Mission du Bicentenaire, du Centre de recherches d'Histoire nord-américaine et de l'Ambassade des Etats-Unis. 1990. Pp. 221. 145 fr.

MARIE-CECILE REVAUGER, En Amérique et en France: Le Franc-Maçon dans la cité. MARIE-FRANCE TOINET and PAUL TOINET, La Presse française et la Constitution américaine. HOERST RÖSSLER, British Chartism, American Republican Institutions, and Constitutions. JACQUES PORTES, Jacques-Pierre Brissot et les Etats-Unis. FRANÇOISE MALET, L'Influence américaine sur un contre-révolutionnaire: Guillaume Hyde de Neuville. CHRISTIAN LERAT, Condorcet sur Franklin: Un éloge funèbre ou un auto-portrait? RICHARD HOWARD, La Naissance du politique. DENIS LACORNE, Les Infortunes de la vertu. LAWRENCE KAPLAN, Economic Republicanism and the Relative Autonomy of the State. M.-J. ROSSIGNOL, L'Obsession de la conspiration, ou toute la vérité sur l'influence française aux Etats-Unis. RONALD CREAGH, The Revolution That Failed: The Cult of Reason in Young America. SIMONE MEYSONNIER, Le Libéralisme anti-capitaliste du XVIII^e siècle en France. FLORENCE GAUTHIER, Droit naturel et cosmopolitique. GUY-ROBERT IKNI, La Critique populaire du libéralisme économique pendant la Révolution française. RICHARD MCMASTER, Mennonites in the American Revolution. JEANNE-HENRIETTE LOUIS, La Pétition de J. de Marcillac et de W. et B.

Roth à l'Assemblée Nationale. LAURIE WOLBERG, Quaker Women in the American Revolution.

B. J. C. MCKERCHER, editor. *Anglo-American Relations in the 1920s: The Struggle for Supremacy*. Foreword by D. CAMERON WATT. Edmonton: University of Alberta Press. 1990. Pp. xiii, 242. \$37.50.

B. J. C. MCKERCHER, Introduction. GEORGE W. EGERTON, Ideology, Diplomacy, and International Organisation: Wilsonism and the League of Nations in Anglo-American Relations, 1918–1920. JOHN R. FERRIS, The Symbol and the Substance of Seapower: Great Britain, the United States, and the One-Power Standard, 1919–1921. B. J. C. MCKERCHER, Between Two Giants: Canada, the Coolidge Conference, and Anglo-American Relations in 1927. KATHLEEN BURK, The House of Morgan in Financial Diplomacy, 1920–1930. ROBERTA ALBERT DAYER, Anglo-American Monetary Policy and Rivalry in Europe and the Far East, 1919–1931. BENJAMIN D. RHODES, The Image of Britain in the United States, 1919–1929: A Contentious Relative and Rival. B. J. C. MCKERCHER, "The Deep and Latent Distrust": The British Official Mind and the United States, 1919–1929.

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JOHN R. GILLIS, editor. *The Militarization of the Western World*. New Brunswick, N.J.: Rutgers University Press. 1989. Pp. ix, 216. Cloth \$35.00, paper \$12.00.

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Communications

A communication will be considered only if it relates to an article or review published in this journal; publication is solely at the editor's discretion. Letters may not exceed seven hundred words for reviews and one thousand words for articles. They should be submitted in duplicate, typed double-spaced with wide margins, and headed "To the Editor."

for a culture that encourages people to write fine history books rather than burn canoes.

JOHN C. MOORE
Hofstra University

Peter Novick does not wish to respond.

THE EDITOR

ARTICLES

TO THE EDITOR:

The recent discussion of "objectivity" [AHR, 96 (June 1991)] was distressing. First of all, much of it was based on two extremes, objectivity and non-objectivity, whereas every practicing historian knows that in history, it is a matter of greater or lesser objectivity. Even the most relativist historians would surely insist to their students that 1789 is a "more objective" date for the French Revolution than 1879. On the other hand, surely no thinking person could claim to know anything completely objectively, could claim to have written the final, all-inclusive history, after which there is nothing left to be said.

Most distressing of all was Peter Novick's response, where he describes his *Wissenschaftliche* practice as no more than a concession to the peculiarities of our culture. He says he observes the rules of historical methodology to gain credibility. Credibility for what? That his argument is more true or more objective than rival arguments? Similarly, he wants his work to be suggestive, fruitful, even "edifying." Suggestive of what? What kind of fruit? Edifying in what sense? If the answers to these questions are not truth or virtue (embarrassingly passé), what are they?

At their worst, Novick's remarks could mean that he seeks only credibility for himself, that he wants only to show that he can play this pointless game better than most? From this point of view, winning the Beveridge Award means no more than a Kwakiutl's bonfire of canoes.

Faced with this fruitless exchange, I can only find consolation in this, that there is something to be said

REVIEWS OF BOOKS

TO THE EDITOR:

In his review of my book, *The Relations of Russia and the Ukraine with the Crimean Khanate in the Middle of the Seventeenth Century* (AHR, 95 [April 1990]: 546), Alan W. Fisher reproaches me with the lack of a new methodological approach to the problem. But it is precisely in its methodology that the work has some new features: for instance, in my analysis of the autonomy of the Ukraine within Russia and my analysis of the foreign relations of the Crimean khanate, which is considered as a practically independent participant in European foreign relations. Naturally, I retained a number of the methodological principles of my predecessors, as these principles are fully corroborated by factual material (for example, the military activity of the Crimean Tatars is explained by the extensive character of nomadic cattle breeding).

Fisher attributes to me the following ideas, which I have never expressed: "Sanin asserts that Russian and Ukrainian achievements and Crimean Tatar and Ottoman failures were all positive developments for the future of East Europe. An inevitable outcome of this approach is the further assertion that Tatar and Ottoman developments are historically significant only to the extent that they influence Russia and Ukraine." There is nothing of the kind in the book. I write only about the fact that the joint military and diplomatic actions of Russia and the Ukraine were aimed at strengthening the act of their reunification, at defending their lands against invasion by the Ottoman empire and Poland. Those joint actions promoted their cultural, economic, and social devel-

opment. That struggle was tightly interlaced with the international situation in Europe, which was determined by the French-Austrian competition on the one hand and by the danger of Ottoman invasion on the other. My idea that the policy of Russia and the Ukraine was an integral part of the European politics of the seventeenth century is not a new one. Earlier Russian and European historians have asserted the same (B. P. Porshnev, Z. Wójcik, G. Stökl). The aim of my work was to track down this link using the specific material on the middle of the seventeenth century.

The reviewer misrepresents one of the fundamental issues in the book. I do not reject the notion that the Tatars defended their state and society and had their own "tasks"; on the contrary, I try to prove that very thing. I assert that the Crimean khanate had considerable independence in the sphere of domestic and foreign policy and that it was far from being always subordinated to Istanbul. The principles of the khanate's foreign policy were aimed mostly at a mutual weakening of Russia, Poland, and the Ukraine. The goal of this foreign policy was the seizure of the Ukrainian and South Russian lands.

Let me also dispute the reviewer's assertion that "Sanin produced no startling evidence; his conclusions remain traditional." At the risk of appearing immodest, I would like to point out that my book is the first to trace the dynamics of Russian-Crimean relations, specifically, their development in connection with the Ukraine's unification with Russia, from the attempts to create a Russian-khanate alliance against Poland to an armed struggle against the Tatars. I studied the diplomatic activities of Bohdan Khmelnytsky after the reunification of the Ukraine and Russia, including its diplomatic contacts with the Crimean khanate, Sweden, Transylvania, Poland, and the Ottoman empire. I showed that those contacts did not have the goal of undermining the reunification. Khmelnytsky's idea of creating a coalition of states opposed to Poland had its supporters at the tsar's court. With the passage of time, that idea became a kind of "reserve variant" of Russian foreign policy. I have tracked the dynamics of the military activities in the Ukraine in connection with its inclusion into the Russian defense system.

I am thankful to Fisher for referring me to the book by William H. McNeill. The reviewer reproaches me with my failure to use the works written by some other authors (Alexandre Bennigsen, Gilles Veinstein, Halil Inalcik, and Metin Kunt). Yes, these are serious studies, just like the book *The Crimean Tatars* by Alan W. Fisher himself. But these works do not touch specifically on the issues in my book. They touch on the history of the Ottoman empire in general or the Crimean khanate or, finally, the history of Islam or of Islam in the USSR. However, I shall try to use their observations in my future work.

The reviewer is evidently careless. He states that on page 241 I write that the problems of the reunification of the Ukraine and Byelorussia with Russia—

moving the Russian borders to the south—were "largely accomplished in the seventeenth century, to 'everyone's benefit.'" There is nothing of the kind in my book. There is a table of the Tatars' raids on page 241. To my mind, the reunification was not completed in the seventeenth century at all. That process was "largely" over only in 1793 as a result of the second partition of Poland. In the seventeenth century, I suggest, the process became irreversible. Similarly, the process of moving the Russian borders to the Black Sea "largely" ended only in 1783 with the annexation of the Crimean khanate. I write that in the seventeenth century the Russian borders only came close to the khanate's borders, and the struggle against the khanate began changing from a defensive to an offense one (pp. 233, 236).

The reviewer denies me membership in a "truly international historical profession." Such pointless labeling does not do credit to anyone.

GENNADY ALEXANDROVICH SANIN
Institut Istorii SSSR AN SSSR,
Moscow

ALAN FISHER REPLIES:

I am pleased to see that our *AHR* reviews are now read and responded to in the USSR. Contacts with our Soviet colleagues are to be encouraged. I ask only that interested scholars read the book themselves and come to their own conclusions. My statement about the international scholarly community was not meant as a slight to Sanin's skills and qualifications; rather, I was alluding to the fact that his work, to date, and the book in question, does not reflect participation (through reading and citing) of historians much beyond his Soviet and East European colleagues.

While one may have expected that junior historians would have been very cautious in their use of Western sources, secondary and primary, senior historians are expected to be models that they could/should follow. Sanin, despite his interesting and important international subject, and very important contributions of primary materials, in referring to secondary historical works cites only his Soviet predecessors, or Polish and Ukrainian and some German historical works, the most recent of the latter a very general study by Richard Peters, *Die Geschichte der Türken* (1961) and Günther Stökl's 1965 *Russische Geschichte: Von den Anfängen bis zur Gegenwart*. And these are used only in his historiographical introduction. The text of the book shows no interest in or knowledge of the broader (international) historical profession. Surely, a study of international relations, contributed to by Turks (Ottomans) and Tatars, might well be expected to incorporate the work on the subject by Turkish and Tatar scholars at the least and European and American historians where appropriate. As Sanin says in his letter, the events and developments were important in European history, and European scholars' treatment of the mid-seventeenth century would

be informative, both in terms of content and methodology.

On the specific subject of this book, Metin Kunt and Halil Inalcik have written on Ottoman northern policies in the sixteenth and seventeenth centuries. Alexandre Bennigsen published Tatar diplomatic correspondence from the time period in question, and Gilles Veinstein has written extensively on the Crimea, the Ukraine, and Moldavia. These scholars' work is surely as relevant to Sanin's subject as is that of Marx and Engels (see his note 65) and Lenin (notes 67–68).

There are certainly American historians of the United States who are not influenced by "foreign" historians' work, but any study of international relations focusing on the United States that did not incorporate (or at least note in passing) the scholarship of historians from the other areas of concern should be characterized in the same way.

With everything else changing rapidly in the USSR, I hope very much for a change in the nature of historical writing from our Soviet colleagues; there is no reason why they should not be significant members of the international profession.

ALAN FISHER

Michigan State University

TO THE EDITOR:

Reviewing my book, *Profits, Power, and Prohibition*, K. Austin Kerr correctly observes [*AHR*, 96 (February 1991): 250] that I view ideas of status anxiety and status revolution as inadequate to explain the history of America's temperance and prohibition movement. Otherwise, he seems to have misunderstood the book's purpose. It was not my intention to write a history of the legions of the faithful who joined the Cold Water Army's crusade. Rather, I devoted my attention to the roles played by the wealthy and powerful in shaping the antidrink movement. Throughout the movement's history, such people played decisive roles in shaping the political destiny of drink reform. Even Benjamin Rush, the Urban II of the first temperance crusade, mixed his "humanitarian" concerns with a very strong dose of economic (capitalist) self-interest. So too did the agricultural societies—like those of Litchfield, Connecticut, and Northumberland, New York—out of which emerged the earliest temperance societies. The same pattern holds true for the early organizations devoted to mechanical improvements. By the 1830s, with hundreds of such temperance organizations in existence, a U.S. Temperance Society was established as a result of the efforts of an elite body of businessmen—whose ambition for the new nation was national economic expansion under capitalist enterprise—a vision that excited the entrepreneurial spirit that dwelt within the hearts of myriad local temperance societies. This pattern of political dominance of the national movement held true throughout the movement's history.

This is not to say that capitalist leadership pro-

voked no opposition from the rank and file. Just the opposite, and my book takes up that challenge in its manifold expressions, principal of which were, in the antebellum period, Washingtonian total abstinence societies, Good Templars, and similar fraternal groups organized within the artisan and working classes. In the post-bellum years, until the turn of the century, the Prohibition party, which in its most radical form embraced Frances Willard and her WCTU followers, served as the vehicle of middle-class temperance sentiment's opposition to the course charted by its capitalist leadership. At the heart of this opposition was the not inaccurate perception that capitalist industrialization was ruining the entrepreneurial middle class. These Prohibitionists charged that the liquor traffic had corrupted politics and exposed the nation to "rapacious class interests" by proletarianizing workers with drink, all the while gathering the nation's capital surplus to itself. By the century's turn, however, such opposition as remained was vestigial. Thanks to a metropolitan elite, based largely in the northeast, that organized the Committee of Fifty to Investigate the Liquor Traffic, the movement had acquired a world view that all but assured middle-class acceptance of a corporation-dominated social order. Despite its successful repression of temperance dissenters, the metropolitan capitalist program to destroy the (working-class) saloon foundered for reasons amply set forth in *Profits*.

Responding to charges of methodological simplicity and factual error and distortion is a tedious and unproductive exercise for authors and readers alike. Disposing of the former allegation is rather like searching for Bishop Berkeley's black cat: Kerr presents no evidence for its existence apart from his own perception of it. Hence response is not possible. Of the other allegations Kerr makes, three deserve comment. First, regarding increases in Prohibition party votes, I am pleased Kerr agrees that, whatever the number, these votes "posed a threat" to the political order's stability. (My statement is amply documented at note 27, chapter 5; Kerr furnishes no source.) Concerning the Anti-Saloon League, the "small potatoes" reference is to the national officers and not to the league as a whole. Neither do I "assert that Peter Odegard was wrong" about the national league's middle-class support. Rather, the league's records show this support not to have been so broad and deep as Odegard supposed. In the matter of the Zanesville ice dealer, Kerr is again mistaken. The reference is to the extent of small business support of the "Widow's Mite" the league was fond of attributing to the fund's namesake.

A clue to Kerr's systematic misrepresentation of *Profits* can be found in his concluding paragraph, wherein he alleges I write in a "larger tradition of denial"—something akin to an alleged symptom of alcoholic addiction—of the "human wastage" wrought by drink. This last gratuitous slur ignores the final sentence of my book, which deplores the

failure of our nation ever to inquire into the reasons for the persistent demand in this country for drugs and drink but rather contents itself with zealous and sterile crusades for repression and prohibition—all the while “denying” that what passes for moral reform in a society such as ours is itself an exercise in “human wastage.” It should not surprise anyone that Kerr, who can regard the suppression by the state of a vital social institution as merely a “systematic” effort to correct “alcohol misuse,” would toss out the baby with the bath water at every opportunity.

JOHN J. RUMBARGER
Washington, D.C.

K. Austin Kerr does not wish to reply.

THE EDITOR

TO THE EDITOR:

I want to comment on sociologist J. William Gibson's review of Mark Clodfelter's book *The Limits of Air Power* (AHR, 96 [February 1991]: 291–92). Facts are the lifeblood of history. In the first paragraph of Gibson's review, there are three factual errors. First, Rolling Thunder, the U.S. bombing of North Vietnam, began on March 2, 1965, not in April. Second, Linebacker I ran from May 8 to October 23, 1972, not from April to October. Third, Linebacker II occurred an entire year earlier than Gibson seems to understand. These errors might be overlooked . . . or blamed on editors . . . except that part of Gibson's critique of *The Limits of Air Power* is that Clodfelter did not cite Gibson's book, *The Perfect War: Technowar in Vietnam* (1986). This would lead one to believe that Gibson is a recognized authority and that *Technowar*, whatever else its merits, contains an authoritative account of the air war over Indochina. Neither is the case.

Gibson claims that “none of Clodfelter's findings are new” and intimates that he has already covered this subject thoroughly in *Technowar*. Had Clodfelter's research extended to Gibson's book, he would have examined a 24-page chapter on bombing North Vietnam, Chapter 9, “Air War over North Vietnam: Bombing as Communication.” He would have found that Gibson's research included documents drawn from volumes 3 and 4 of Senator Mike Gravel's edition of *The Pentagon Papers*; estimates on bomb tonnages derived from Ralph Littauer and Norman Uphoff's 1972 edition of the Cornell University Air War Study Group's *Air War in Indochina*; references to two issues of *Indochina Newsletter*; a published Military Assistance Command Vietnam *Order of Battle*; Gloria Emerson's *Winners and Losers*; and an article by Jessica Benjamin, “The Bonds of Love: Rational Violence and Erotic Domination,” from the journal *Feminist Studies*. With the exception of Emerson's politicized diatribe and the article on erotic domination, Gibson's sources are standard starting points for investigating the air war. But Clodfelter's sources went beyond

these to include documents declassified by the U.S. Air Force, Department of State, CIA, and three presidential libraries; and personal interviews with individuals such as former Secretary of State Dean Rusk, Secretary of Defense Robert S. McNamara, Seventh Air Force commanders Joseph Moore and John Vogt, as well as numerous pilots and navigators. To be sure, Gibson's book includes several other chapters on the air war, each with substantially more references, but there is a remarkable lack of primary sources among them.

Gibson claims that “Clodfelter never saw that the industrial approach to bombing, with its constant emphasis on using ‘air assets’ to increase the air force and navy ‘sortie rates’ . . . led to constant bombing attacks against anything that vaguely resembled an ‘industrial target.’” True enough, Clodfelter does not interpret the air war as a militarized manifestation of a perverted capitalist enterprise, as does Gibson. That is not to say that Gibson's thesis is not without value (I find it fascinating); it is simply not Clodfelter's thesis. What Clodfelter does do is to show how parochial institutional interests drove competition between the air force and the navy to achieve higher sortie rates.

Later in his review, Gibson would have one believe that Clodfelter's book is a defense of the air force's conduct of its aerial campaigns. He cleverly focuses on Clodfelter's citing of Trong Nhu Tang's account in *Vietcong Memoir* of what it was like to be close to a B-52 strike. Clodfelter does not base his argument on Trong's being understandably frightened by several hundred 500-pound bombs going off nearby. Rather, Clodfelter offers a well-documented argument that makes the case that air power leaders, especially air force generals, were blinded by their commitment to strategic bombing doctrine.

Linebacker I, the air campaign in response to North Vietnam's massive invasion of South Vietnam in the spring of 1972, worked not because B-52 strikes can stir the soul and boggle the mind but because massed forces offer excellent targets for saturation bombing and because precision-guided munitions and mining isolated North Vietnam from outside sources of the supplies needed to keep fourteen North Vietnamese divisions stocked with fuel and ammunition. The core of Clodfelter's thesis, however, is that while Linebacker I and Linebacker II (the bombing of North Vietnam for eleven days in December 1972) succeeded in getting North Vietnam to sign a cease-fire agreement, they did not “win” the war. Indeed, the point of *The Limits of Air Power* is that conventional bombing of North Vietnam was inappropriate in a war that was essentially a civil war and an insurrection. If Gibson would read Clodfelter, he would find they agree more than they disagree.

But one is left to wonder if the reviewer read Clodfelter at all, or did he decide that since the author was an instructor at the U.S. Air Force Academy that his book would be a defense of air power and the way the air force fought the war?

The Limits of Air Power, a revision of Clodfelter's doctoral dissertation at the University of North Carolina, is a well-researched, solidly documented, balanced critique of the air force and the way it conducted the air war against North Vietnam. Far from being uncritical, it devastates the narrow-mindedness with which air power leaders (especially USAF generals) approached a war they did not, indeed could not, comprehend. Clodfelter, an air force major, ran considerable professional risks in publishing this book. It is, and will remain, the standard work in the field.

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J. WILLIAM GIBSON REPLIES:

I regret the opening sentence of my review in which I placed Linebacker II in December 1973 rather than December 1972. I did not make the same error in either my book, *The Perfect War: Technowar in Vietnam* (1986), or later on in my review of Clodfelter's *Limits of Air Power* (1989). However, I have no other concessions to make to Major Earl Tilford, Jr., formerly of the U.S. Air Force's Center for Aerospace Doctrine, Research, and Education (CADRE) and a colleague of Captain Clodfelter who reviewed his manuscript before publication (Clodfelter, p. xiv).

Yes, Major Tilford, I read *The Limits of Air Power* very carefully. However, although I saw quotations from official documents I had not seen before, I did not learn anything new about the air war over North Vietnam. To the contrary, the "new" memos and studies cited by Clodfelter look like the documents collected in *The Pentagon Papers* (1972) and in previous military accounts of the air war. Said another way, both the old and the "new" studies freshly pulled from the archives are part of the same official discourse about bombing North Vietnam.

I also saw some passages in *The Limits of Air Power* that were very similar to passages I wrote or cited years earlier. For example, in *The Perfect War* I emphasized that many high-level officials were so enchanted with their technologically advanced air power that in effect it became *inconceivable* to them that they could not force North Vietnam into submission and acknowledgement of American superiority. One of my major methods for making this argument was close textual analysis of official documents, frequently italicizing key words.

When I read Clodfelter, I found a quotation from a memo USAF Major General Ginsburgh wrote to Walt Rostow saying, "The bombing of the North *must* have some effect on the war in the South and the punitive effects in the North *must* have some influence in measuring the course of the South against the costs in the North" (Clodfelter, p. 100). Or, to take another instance, I once commented on intelligence estimates that the Vietcong and NVA forces in the South only needed twelve tons a day of supplies from the North

by noting that this tonnage estimate was so small that even if it were multiplied several times, it could be transported by a relatively small number of trucks that would be nearly impossible to stop by bombing. Years later, Clodfelter made a very similar comment about how few trucks were required, but his footnote cited air force documents. If this kind of research strikes Major Tilford as profoundly original, he should understand that other people may well view it differently.

Major Tilford contrasts Clodfelter's "original" research to my "secondary" research, drawing particular attention to the presence of a feminist scholar—the implication clearly being that a feminist scholar of sadomasochism simply cannot have anything serious to say about bombing. I cite Jessica Benjamin's article, "The Bonds of Love: Rational Violence and Erotic Domination" (*Feminist Studies*, 6 [Spring 1980]), because she astutely notes that the violence committed by a sadist has to be limited: if the sadist kills the victim, then the victim can no longer recognize the sadist's superior power and thus give the sadist pleasure.

This argument clarifies the debate among war managers about how much bombing should be escalated. At one level, they wanted escalated bombing to make the North Vietnamese leaders see what Maxwell Taylor called a "vision of the inevitable, ultimate destruction," while at the same time war managers feared escalation would "kill the hostage" inside the "Hanoi donut." If the North Vietnamese leaders were dead, then they could not recognize American superiority. Thus the idea behind escalated bombing was to torture the Vietnamese into submission. Interestingly enough, Clodfelter tries to prove the efficacy of the 1972 Linebacker attack on Hanoi by quoting from a former Vietcong minister of justice who defecated when under B-52 bombardment. This "proof" takes the place of an investigation of whether or not the treaty signed in January 1973 differed significantly from the draft prepared in the fall of 1972.

Finally, I would like to say that in my original review of Clodfelter's book, I did not complain that my own work had been neither cited nor listed in the bibliography. I assumed that this was normal academic politics. Instead, I noted that a systematic analysis of air war had been ignored. An editor at the *American Historical Review* then asked for clarification.

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NOTE FROM THE EDITOR:

Gibson's original review stated, "a book published in 1986, but curiously not cited, systematically analyzed [the] air war's conceptual and operational structures and why the system failed." The Assistant Editor asked for the name of that book. Gibson wrote on the galley, "It is my book, James William Gibson, *The Perfect War: Technowar in Vietnam*."

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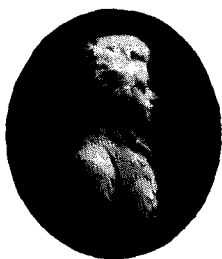
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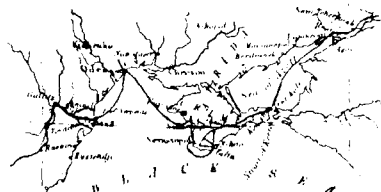
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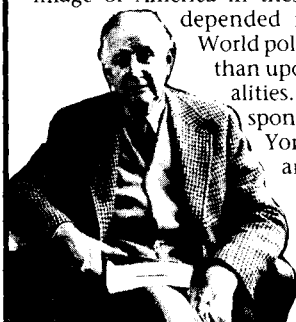
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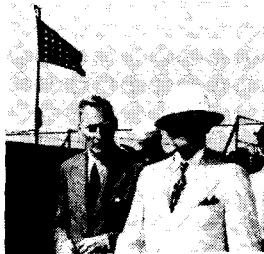
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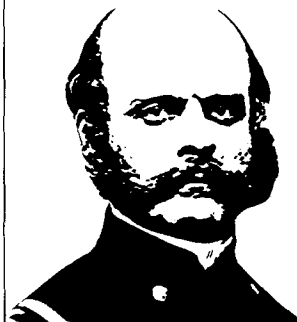
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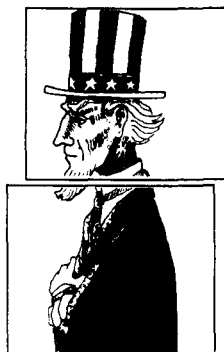
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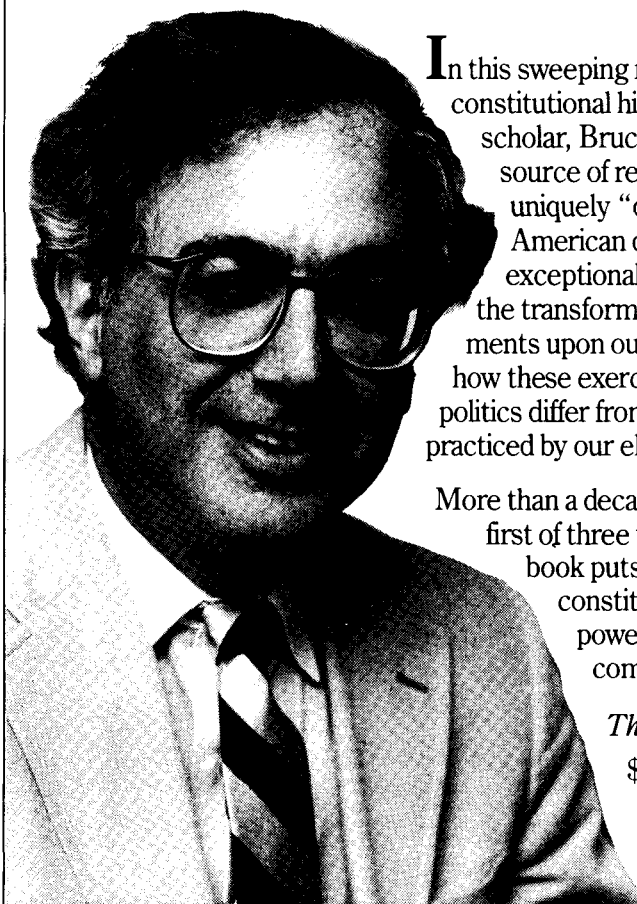
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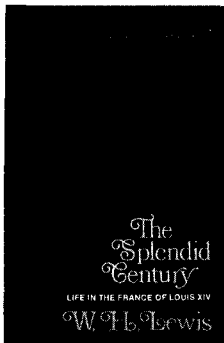
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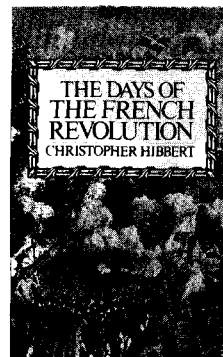
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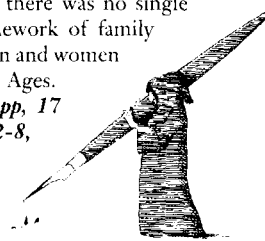
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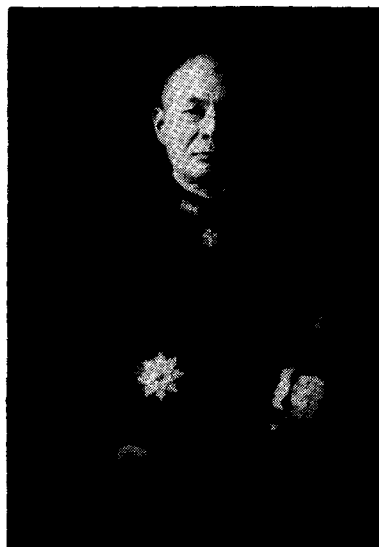
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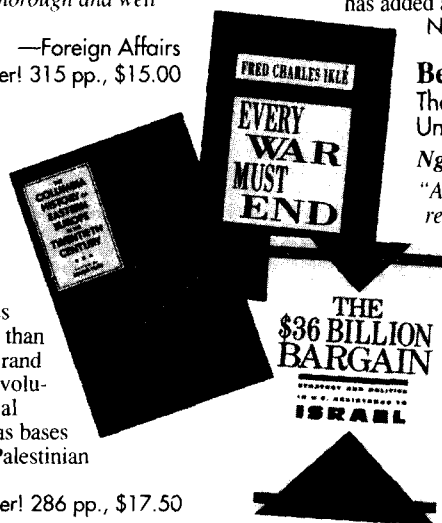
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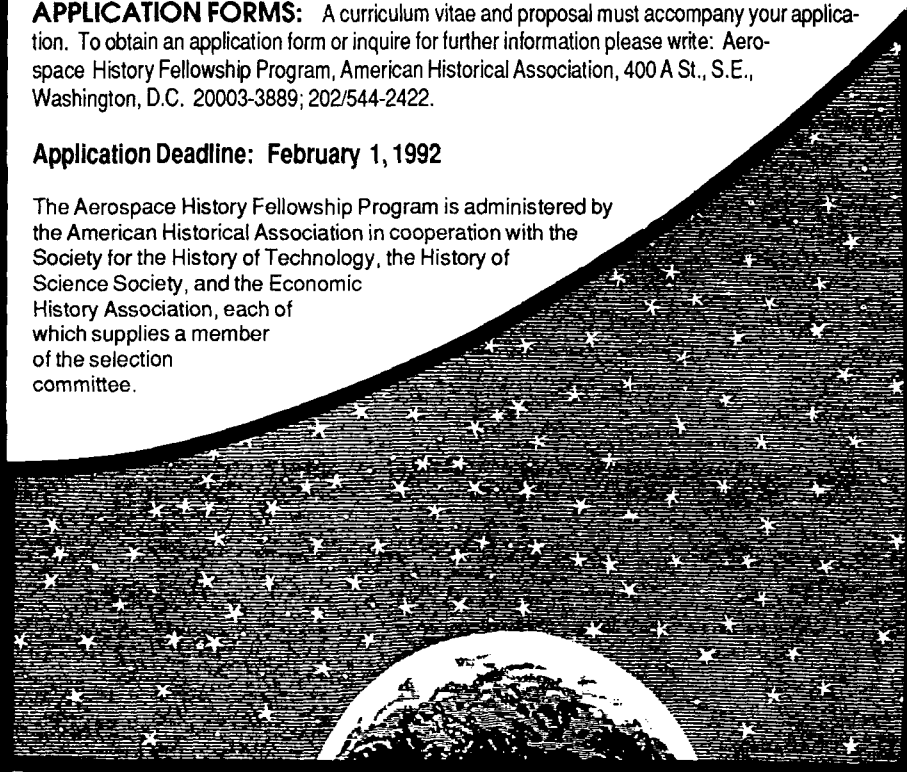
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